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THE ONTOGENETIC SIGNIFICANCE OF INSTINCT, HABIT, AND INTELLIGENCE

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It is notorious that the terms instinct and intelligence are exceptionally ill defined and loosely used. To see this there is no need to take into consideration those many times slain superstitions which regard instinct as a sort of *clairvoyance* or skill peculiar to animals, and intelligence or reason as the special property of man. Even in the best psychological thought we find at this point a disagreement so extreme that it very strongly suggests some fundamental misconception. The situation is very apparent in the reports of the symposium of 1910, on instinct and intelligence[1] and the symposium of 1919 on instinct and the unconscious,[2] held at joint meetings of the Aristotelian Society and the British Psychological Society, where the most divergent definitions were advanced. It is, indeed, one of the striking characteristics of modern psychology.

Possibly a question of definition may seem of secondary importance. It may be held that empirical research is all that matters, and that if psychologists will look after the facts, the categories will take care of themselves. While there may be some ground for such optimism, we must not forget that loosely defined categories have many times led to pseudo-thinking and sham research. Scientific progress depends, to be sure, on the availability of empirical data,

[1] *Brit. J. of Psychol.*, 1910, 3.
[2] *Brit. J. of Psychol.*, 1919, 10.

but it becomes impossible unless the data are accurately classified and assimilated.

Clear cut generalizations seem peculiarly desirable in analyzing the mental life of individual human beings. In itself the subject is one of great complexity and liable to many kinds of prejudices. Moreover the mass of data on hand is already enormous and is rapidly growing. Abnormal and applied psychology are well nigh swamping us with detailed facts. But these facts are usually ill-coördinated and formulated in an *ad hoc* terminology, so that inevitably their scientific value is reduced to a minimum. There is a real danger that they may confuse rather than enlighten. For this reason it is a vitally important task to organize and assimilate this great body of detail. Only by so doing can we hope to reach a rounded and authoritative understanding of the mental life of the individual.

I believe that a real step in this direction will be taken when we arrive at a workable and consistent conception of the three fundamental categories of instinct, habit, and intelligence, and their rôle in the ontogenetic series. Until this has been done there is imminent danger that any discussion of general psychological principles will land us in a quagmire of verbalism.

I. Instinct

If we regard instinct as an hereditary mode of response it may seem that we have arrived, not perhaps at a complete definition, but at least at a characterization that is quite unambiguous as far as it goes. But the deep and far-reaching differences among those who hold to this general notion go to show that such is not the case. I think that our best chance of coming to a clear understanding of what is involved in the idea of hereditary response will be to leave on one side the numerous and conflicting theories, many of which are hardly worthy of consideration, and to study the empirical methods employed in picking out the instincts.

The experimental worker has to tease out the instinctive factors from the complex mass of total behavior. There is

no one criterion which will yield certain results, and with all the criteria at his command he is far from being able to reach satisfactory and well-founded conclusions. As Thorndike frankly admits,[1] the selection of hereditary or original elements in complex behavior often depends as much on intuition as anything else. Still the experimentalist is definitely looking for something among a confused mass of data. He may not always trouble to formulate just what it is he is seeking, and sometimes one suspects that he hardly knows. But the fact remains that a critical consideration of some of the empirical methods now in use is the most hopeful means of clearing up the definition of instinct as hereditary response.

A. It is said that the hereditary responses will not be peculiar to the individual, but common to the biological group of which the individual is a member. In experimental practice such a criterion cannot carry us far. It warns us against classifying as instincts any functional peculiarities, whether habitual in the ordinary sense, or pathological, of any individual whose behavior we may be investigating, and this is about all. Its insufficiency is clearly seen from two sets of considerations. On the one hand, most of the more complex instinctive reactions of any individual will be in a measure idiosyncratic. And on the other, many acquired responses will be made common to a biological group by social heredity or its analog. Rivers contends that suggestion plays a very large part in determining behavior on all levels below that of civilized mankind.[2] He instances Polynesians, who seem able to coöperate effectively in various social tasks, such as handling boats, or engaging in warfare, without any definite leadership whatever. Perhaps the alleged telepathic dissemination of news among savage populations also depends upon suggestion. Thus many forms of behavior which on this criterion would seem to be hereditary are probably not true instincts at all. Rivers even goes so far as to advance the opinion that the highly standardized

[1] E. L. Thorndike, 'Educational Psychology,' I., pp. 38–9.

[2] W. H. R. Rivers, 'Instinct and the Unconscious,' Ch. XII.

reactions of such insects as the bee, the ant and the yucca moth, which certainly have all the appearance of instincts, arise out of suggestion. So it is evident that the fact that a behavior is common to a biological group by no means enables us to say that it is instinctive. Nevertheless, this criterion is important for the present discussion in that it suggests that hereditary modes of response are somehow dependent on inherited structure, identity in regard to which is the basis of biological groupings.

B. It is said that instincts possess relatively high efficiency apart from use and learning. This is the most celebrated of all alleged characteristics of instinct, and perhaps the poorest of all criteria. Assuredly we cannot say with Driesch that an instinct is perfect apart from use. And I doubt whether Lloyd Morgan's emendation to the effect that instincts are practically serviceable apart from use will be of much value. Breed's investigation of the pecking reaction shows that day-old chicks have a success of only 20 percent the first day.[1] And probably the initial success of more complex instincts would be far less. Moreover the notion of practical serviceability seems to involve an assumption that all instincts are adaptive. By serviceability, I take it, is understood preservation value. And we may say with considerable certainty that there are many instincts which are not in any immediate sense adaptive. Thus the criterion of high initial efficiency can apply only to the simpler adaptive instincts.

C. It is said that an act performed without any instruction merely on the occurrence of the appropriate stimuli will be instinctive. Doubtless this criterion is of great value in dealing with the simpler instincts, but it becomes very ambiguous when we come to complicated responses. If we suppose with James that speech is instinctive, where shall we look to find the first occurrence of the reaction? Will the first speech be the first intelligible word? Obviously not, for this is the result of long encouragement and much trial and error. To find literally the first occurrence of speech we would have to go back to the first vocalization, the birth-

[1] Breed, Behavior Monograph Ser. No. 1, p. 31.

cry—that is, to something that is not speech at all in the ordinary sense. Or again, we cannot say that the first walking is the first step in an erect position. To eliminate all the results of practice we would have to go back to the first use of the locomotive apparatus, and this would hardly be walking.

Ambiguous as the notion of initial performance must be when applied to complex acts, we may find here a very definite suggestion of the true nature of hereditary response. Employing the criterion of initial performance apart from instruction, we arrive simply at the elementary tendency of structure to function. Look for the first speech and we are carried back to the birth-cry; look for the first walking, and we are carried back to the infant's random kicking motions. On these original elements all later efficiency must be grafted. We have seen that the proposal to regard instincts as responses common to a biological group points in the same direction. Thus the attempt to clear up the definition of instinct as hereditary response turns on the relation of inherited structure to function.

We may think of an animal as a more or less elaborate system of systems of cells. The variety of its activities will in general correspond with the grade of complexity of its organized system of systems. Ignoring for the time being any modifications which may be brought about by practice, we may say that the cells themselves, together with their systematic connections, constitute the individual's inheritance.

This conception at once enables us to reduce function down to its ultimate elements, at least in a schematic way. Let us suppose that a stimulus *S* affects a receptor. This receptor will itself be a cell system which we may speak of as *a*. Now by virtue of the relations among the cell systems constituting the organism, if circumstances are favorable, the release of energy in *a* will involve a series of cell systems, *a b c d e f g* . . . which will become active in turn. Let us say that the serial functioning of the systems *a b c d e f g* . . . constitute the response *R*. Now the occurrence

of *R* depends on two factors—the presence of *S* and the availability of every element in the series *a b c d e f g* . . . . Suppose that *S* occurs when the systems *d e f* are actually functioning as elements in another series *m n o d e f x y z* . . . , which has been initiated by a stimulus S_1, and which constitutes the response R_1, or when *d e f* are fatigued, or for the moment rendered refractory. Then it is clear that *R* cannot occur, even though *S* is present. To take an example, the pecking reaction will not occur in spite of the presence of appropriate stimuli, if the organic structures are taken up with a fear response.

The probability of the appearance of *R* when *S* occurs depends on several factors which can now be enumerated.

1. Other things being equal, the shorter the series of cell systems whose serial activity constitutes *R*, the higher is the degree of probability that *R* will occur when *S* is present. This is an exceedingly elementary consideration, for the smaller the number of structures involved in producing *R*, the less likely will it be that any of them will be unavailable when *S* is present. Here we see a reason for the high stability of many very simple responses. The more complex the response, the more structures it involves, the more liable it becomes to be rendered impossible by interruption.

2. Again the systems *d e f* may be so intimately integrated with the series *m n o d e f x y z* . . . that even though they have begun to function as elements in the response *R*, the occurrence of S_1 may switch them out of the series *a b c d e f g* . . . , and check the response *R* in favor of the response R_1. Some complex behaviors are exceedingly easy to disturb, others again can only be interrupted by violent fear or rage, while still others, such as the patellar and pupillary reflexes, depend on structural series so simple and consolidated that they cannot be interrupted by any normal agency once the stimulus is given.

3. A given response may depend upon a series of cell systems that is not highly integrated, and so in theory be very easy of interruption. Yet the prepotent stimuli may

occur only with great rarity. Eating raw oysters is in general a complex behavior not very highly consolidated. But I have a friend who could be interrupted in the proceeding by few stimuli more common than the trump of doom.

Instinct, then, is a response determined wholly by inherited structure. The degree of probability that the response will occur when the appropriate stimulus is given depends precisely on the conditions just analyzed. We arrive then at Thorndike's notion of an original tendency as constituted by 'a situation, a response, and a degree of probability that apart from training the latter will happen when the former does.'

It may be objected that this whole discussion is too theoretical. This is true if we mean that psychology is far from having developed an experimental technique that will enable us to analyze instincts down to situation, response and degree of probability. On the other hand, this notion of instinct is involved in the experimental work already in hand, and it is a clear-cut ideal for objective psychology. I do not think that behaviorism can afford to stop short at anything less than a radical analysis of all the structures involved in instinct and their relation to one another. And such an analysis, it seems to me, would in effect amount to filling in the details of the highly schematic conception I have tried to present.

Let us consider a few of the more immediate systematic consequences of this view.

1. How will it affect our view of the relation between instinct and emotion? As Angell has pointed out, if we accept the somatic theory of the emotions, we are committed to recognize the instinctive basis of emotional reactions.[1] Indeed it may even appear that for us there can be no important distinction at all between instincts and emotions. It is true that inasmuch as certain emotions may be regarded as hereditary modes of response, there will be no systematic

[1] J. R. Angell, 'A Reconsideration of James's Theory of Emotion in the Light of Recent Criticism,' PSYCHOL. REV., 1916, 23.

cleavage between them and instincts. At the same time it is convenient to have both terms for use; so accordingly we may accept Watson's definitions. "We should define instinct as an hereditary pattern reaction, the separate elements of which are movements principally of the striped muscles." [1] "An emotion is an hereditary 'pattern reaction' involving profound changes of the bodily mechanism as a whole, but particularly the visceral and glandular systems." [2]

2. From our point of view there seems to be no distinction save in degree between instinct and reflex. A reflex is a very highly probably instinctive response, usually depending on a simple and highly consolidated structural series. It may be worth noting here that a distinct advantage is gained in dealing with the whole relation of instinctive functions to structure in terms of cell systems primarily rather than in the more specific terms of receptor mechanisms, connective mechanisms and responsive mechanisms. The reason is that certain important reflexes do not seem to depend on the nervous system at all. Such, for example, is the case with the pancreatic secretion, and with the relations set up between the ovaries and the uterus in menstruation. So in general the best mode of statement seems to be that adopted by Marshall. "The reflexes appear to me to be merely instinct actions of thoroughly coördinated minor systems within the whole system; minor systems which have gained a large measure of independence." [3]

II. Habit

The notion of habit seems to be better defined than either instinct or intelligence. At the same time, I cannot feel that the existing analysis is altogether satisfactory. To begin with let us remind ourselves that all we can say for certain about the nature of habit in general is that it is a modification of behavior consequent upon experience or use. This, to be sure, is a description rather than an expla-

[1] John B. Watson, 'Psychology from the Standpoint of a Behaviorist,' p. 231.

[2] Ibid., p. 195.

[3] H. R. Marshall, 'The Relation of Instinct to Intelligence,' *Brit. J. of Psychol.*, 1912, 5, 265.

nation, but it is as far as we can go on firm ground. For the sake of precision of thought it is very important to bear this in mind, especially in view of the popularity of alleged neurological explanations of habit. All of these go beyond the established facts, and involve a speculative element.

The crucial question which I wish to raise here, which also seems to me to be the fundamental question for the whole psychology of habit is this: Is habit organic or functional? Before coming to this there are two points which must be disposed of.

1. What is the relation of habit to instinct? It seems obvious from the outset that habit must be thought of as a modification of instinctive behavior. Habits cannot arise *de novo*, but must be grafted onto behavior already set up. One or two illustrations may serve to make this process clearer. Semon cites the case of hunters attracting stags by imitating the call of members of their species, either male or female, and points out that the older the stag the more difficult does the deception become.[1] The reason is that habits of discrimination have been grafted upon the original instinct. Again, to take an example from human life, the original sexual instinct presumably consists of the orgastic reaction following upon certain tactile stimuli. But on this simple foundation mankind has erected a most complex edifice of habit, so that the civilized human being occupies a large proportion of his time in what is in effect a ritual of sex.

The detailed lines along which habitual modifications of instinctive behavior may take place are far too numerous for consideration here. In general, however, such modifications may follow three lines—they may increase or diminish the degree of probability of an instinctive tendency, they may affect the form of an instinct, or they may bring about consolidations of instinctive tendencies. In human life all three usually occur together. What is much more important is that the general effect of habitual modification is to make

[1] R. Semon, 'Die Mneme,' pp. 207–9.

the instinct more successful. A complex social environment demands the development of considerable skill on the part of the individual to prevent the exercise of original tendencies leading to a capital sentence. For instance if the sexual instinct is to function harmoniously in a civilized society, it is absolutely essential that it be modified, first by what are in essence conditioned reflex methods, and second by becoming consolidated with a mass of æsthetic and other instincts, which themselves have undergone profound habitual changes. So in general habit graduates, adapts and renders skillful the instinctive tendencies. Of course there will be some exceptions to this rule in every life, and wherever failure becomes excessive one of the psycho-neuroses is likely to appear. But the tendency toward better adjustment is usual, and this fact is highly significant.

2. What is the status of the proposed physiological explanations of habit? Of all such attempts, whether they depend on the alleged formation of brain paths, the lowering of synaptic resistances, or retention traces in the neurones, it must be said that while they are ingenious, they are based essentially on ignorance. We know very well that the receptors and effectors are not importantly modified in habit-formations, we do not know that the connective elements are not so modified, so we suppose that they are.

However let us admit, for the sake of argument, that something of the sort does take place. Still we are far from a complete explanation of habit-formation. Let us say that every response leaves a retention trace in the connective mechanisms. It is not every response, however, that tends to become habitual. We have not explained how certain modifications and not others are set up, and, more specifically, why there is a tendency for successful responses to become permanent. Watson attempts to explain this by what he calls the principle of frequency.[1] Suppose that in a given situation there is a repertoire of ten movements which the individual can make, and that only one will lead to a successful result. Then the successful movement will

[1] John B. Watson, 'Behavior,' pp. 262–3.

occur each time a trial is made, while each of the others occurs on an average in only half the attempts. By this scheme Watson proposes to show how the successful response becomes established. Russell's criticism of this suggestion is as follows: "The objection to this view, if taken as the sole explanation, is that no improvement ought to set in till after the *second* trial, whereas experiment shows that already at the second attempt the animal does better than the first time." [1] The weakness of all physiological explanations of habit with which I am familiar is that they assume that modification of behavior depends exclusively or importantly upon repetition. As a matter of fact, behavior may often be modified after a single trial. The effects of success seem quite as important as those of repetition.

Let us now take up the previous question as to whether habit is organic or functional. It is very clear from the two points just discussed, that we cannot point to any close and simple relation between habitual functions and organic structure, such as that between instincts and structure. These difficulties suggest that we may be forced to regard habit as functional. Let us try to make clear just what this would mean.

The best means that has been proposed for separating the functional from the organic, so far as I know, is that which was brought forward in the seminar in logic conducted by the late Professor Royce, at Harvard University. It was suggested that functional factors may be reversible, while organic factors are not. This would certainly seem to indicate a significant difference between instincts and habits. In general the profounder and more radical the modification, the less stable the result. When the habitual modification is no more than a slight improvement in motor coördinations, such as is involved in learning to swim instead of flounder, there is a high degree of stability. On the other hand, an elaborate artificial scheme of social convention may collapse under any stress or even under its own weight. Again, much modern psycho-therapeutic practice is based on

[1] Bertrand Russell, 'The Analysis of Mind,' pp. 52–3.

the belief that habitual maladjustments are in many cases reversible. Instinct, on the contrary, seems to be irreversible. It may undergo profound transformations by superimposed habit, but as we have seen, the essential purpose of such habit-edifices is to give the original tendencies play in complex circumstances. The few alleged cases of the waning of instincts through disuse fail to carry much conviction, and we may safely say that in the great majority of cases nothing short of the atrophy or excision of tissue will eliminate an instinct. Accordingly it seems possible that instinct may be organic and habits functional.

It will be objected that the notion of functional change without any structural modification is incredible. We conceive of behavior as the functioning of a mechanism, and while any mechanism remains totally unaltered we seem bound to believe that it must continue to function in the same way. This, to be sure, is a more or less theoretical consideration, but it is a sound scientific principle. Modification of behavior without any change of structure has rather too much the look of a miracle.

To meet this difficulty I would raise two points.

1. The organic change involved in habit formation is certainly very slight. Manifestly when a new habit is established after ten trials, no evident structural alteration has taken place. Here once more we see the convenience of referring any such change to the nervous system. It is easy to think of an occurrence of which we know practically nothing by direct observation as taking place in the most obscure and delicately adjusted tissues of the body. And furthermore an alteration in nervous integration will certainly be exceedingly slight from the gross anatomical point of view.

2. The hypothetical organic change involved in habit formation seems to be quite unimportant. I can show this best by an illustration from psycho-therapy. Rivers cites a case of claustrophobia which arose out of a suppressed childhood experience of being shut in a narrow hallway with a dog. An attempt to treat such a case by rest, diet, and

so forth, on the assumption that we had to do with a nervous lesion, would probably fail. In theory some lesion may have been present. But even assuming that it could have been dealt with directly, which is far from true, the point for us is that this was quite needless. The maladjustment was successfully treated on purely functional lines by persuading the patient to face his suppressed experience in the belief that it was the cause of his present troubles.

The mere complexity of organic structure is sufficient to explain how a very slight modification may have a far-reaching functional effect. Suppose that a given cell system is a necessary element in thirty responses. Then if it undergoes change, all thirty responses will be modified. This, however, is a matter of theory, as indeed any discussion of the organic basis of habit-formation and the results of habit must be. It would seem that habit is essentially a matter of function.

III. Intelligence

By the intelligence of an individual we mean his total efficiency. By the intelligence of any act we mean its efficiency. Preserving this suggested distinction between general and special intelligence, I wish to try to give the whole conception precision, and to relate it to the treatment of habit and instinct.

The obvious question which the above characterization of intelligence suggests, is: What is efficiency? We cannot, I think, employ self-preservation as a criterion. A human being, in the course of a lifetime, has to do with many problems which are certainly not immediately connected with self-preservation. The concept of survival value, fundamental as it may be for biology, is not sufficiently inclusive for objective psychology. The problems which the environment may set to any individual seem ultimately to depend on one thing, and one only, the range of the individual's sensitivity. We may think of the individual as responding to such environmental factors as may act as stimuli. His general intelligence is the average grade of efficiency with which he is able to meet these problems. And any given

act is intelligent or efficient in proportion as it puts the individual in possession of the stimulus.

It is clear that this is the conception of intelligence that underlies all testing for general mental level. The environment is regarded as a constant, and individuals differ in the degree to which they are able to deal with it as it is mediated to them through the receptor mechanism. Mental tests propose to measure this degree by the application of the method of sampling.

Such a characterization seems to be a correct enough description of the ontogenetic significance of intelligence. If we adopt the phylogenetic point of view, however, there is an added complication which it may be worth while to consider in spite of the fact that it lies outside the strict limits of the present discussion. While we confine ourselves to a group of similar individuals, such as human beings, the environment can be taken as a constant. But if we take in the lower grades of creatures, this is no longer the case, for the conditions of sensitivity vary. The environment does not present at all the same problems to the man as to the rat, because they are not sensitive to the same environmental factors. We can speak with some degree of accuracy of the mental level of individuals of a homogeneous group, where the effective environment can be taken as a constant. But the case is very different when we try to compare the intelligence of creatures of different species, for nothing in the way of a satisfactory scale exists. Nevertheless we may still retain our notion of intelligence as efficiency, though here the grade of efficiency depends on two variable factors instead of one.

I now turn to a consideration of the relation of this idea of intelligence to instinct and habit as I have conceived them. I think it is important that in so doing we bear in mind the distinction between general and special intelligence.

1. *Intelligence and Instinct*

(*a*) It is clear from the foregoing discussion that we may believe the whole attempt to distinguish between intelligent and instinctive acts to be based on a mistaken analysis.

Rivers makes an attempt to draw such a distinction along traditional lines, but stated with more than usual precision. In general, though with certain exceptions, an instinctive act would be what he calls 'protopathic,' that is, it would have the following characteristics: " . . . firstly the absence of exactness of discrimination, of appreciation, and of gradation of response; secondly the character of reacting to conditions with all the energy available; and thirdly the immediate and uncontrolled character of the response." [1] He tells us that suggestion and intelligence are the two agencies which modify protopathic factors, and bring to light the epicritic characteristics of gradation to stimulus and general control and accuracy in responses.[2]

All that this gives us, however, is a scheme of more or less efficient and well-adjusted responses—responses which to a greater or less degree give the individual possession of the stimulus. Moreover the sharp distinction between protopathic and epicritic factors, which Rivers imports into the treatment of instinct and intelligence from Head's research on touch and pressure sense, does not exist here. A response entirely protopathic in Rivers's sense would have a minimal amount of intelligence, that is, efficiency in putting the individual in possession of the stimulus. From such instances up to the most efficiently and perfectly graduated acts there are very many intermediate steps.

(*b*) We sometimes see general intelligence loosely spoken of as an instinct. Obviously this is a misnomer. The only justification for such an idea seems to be that general intelligence seems to be hereditarily determined. The Binet tests and their various revisions claim to give 'native intelligence,' and the results bear out the contention. Moreover the connection between general intelligence and structure is suggested by post mortems of idiots and imbeciles, which have revealed marked modifications in the cortical neurones. We may say then that general intelligence depends on inherited structure. I shall return to this point when dealing with habit.

[1] W. H. R. Rivers, 'Instinct and the Unconscious,' p. 48.
[2] Ibid., pp. 99–100.

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2. *Intelligence and Habit*

(*a*) I have already pointed out that the importance of habit is that it ingrains successful procedures. When the cat learns to escape from the problem box, it renders possible a certain instinctive fulfillment—the consumption of the bait—in spite of environmental complexities. When a man goes through the rigmarole of a formal dinner, his habit constructs are making possible the skillful and adequate satisfaction of a very complex group of instincts. My conclusion would be that special as opposed to general intelligence depends mainly on habit—that is, on functional modifications in the interest of success. General intelligence, we have seen, is contingent in a manner not yet analyzed fully, upon heredity. Now general intelligence, in the sense of any individual's intelligence quotient, is nothing more than the individual's average success. And a moderate intelligence quotient is by no means incompatible with exceptional specialized expertness. This may be brought about by carrying the work of habit formation, *i.e.*, functional modification, to an extreme degree in one special connection, so that a certain group of responses become very highly efficient.

(*b*) One of the most common criticisms of general intelligence tests is that they fail to do justice to the efficiency of many individuals, in that they presuppose the existence of special habits. Specifically this objection is often urged against the use of vocabulary tests in particular and language tests in general. The results of psycho-clinical practice, however, seem in a measure not to sustain this *a priori* objection. In cases cited by Terman and others, the diagnostic value of language tests has been surprisingly great even for individuals who have recently come from non-English-speaking countries.

The reason for this is suggested by our analysis of the relation of instinct to habit. Habit, we have seen, is essentially a functional modification of instinctive behavior. Habits do not come "out of the everywhere into here." And given a good heredity—that is to say a rich and already effective background of instinct—it seems that an individual

can build up an edifice of habit with surprising ease. This is the reason for believing that general intelligence depends on structure. An intelligence quotient may indeed be no more than a statement of the average efficiency of a large number of habits. But such a result is highly significant, probably one of the most significant things we can know about any individual. No amount of training can graft a habit edifice of high average efficiency upon a slight inherited foundation. But given a good heredity proportionately great general efficiency can be developed usually in spite of the accidents of education.

IV. Summary and Conclusions

My object has been to develop a conception of instinct, habit, and intelligence in such a way as to give a very schematic but accurate and clean-cut picture of the mental life of the individual. I can sum up results very briefly.

We may think of the individual as possessing an hereditary or original nature, highly definite and positive of content, and constituted of instincts which depend wholly upon inherited structure. Existing methods of psychological analysis can by no means enable us to isolate all or many human instincts, but the conception of an instinct itself can be made perfectly definite and clear. The instincts constitute the basic tendencies or impulses of the individual, and can be modified but not eliminated by habit. The value of habit is that it adapts original tendencies to a complex environment, and makes action more successful and more intelligent. As McDougall insists, "The human instincts are not suppressed by new forces and incompatible modes of mental adjustment beneath which they crouch like a cage full of wild beasts."[1] Ordinarily they are the foundation of a habit edifice which constitutes a fairly harmonious and efficient personality. This is the normal ontogenetic sequence from which it is the weakness of the Freudian school not sufficiently to distinguish those pathological cases which arise from functional maladjustment.

[1] W. McDougall, 'Instinct and the Unconscious,' *Brit. J. of Psychol.*, 1919, 10, p. 42.

PRACTICAL LOGIC AND COLOR THEORIES

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The psychologists, when they discuss reasoning at all (and some of them hardly give it passing mention), take the ground that the kind of reasoning that interests them is something very different from the cut and dried formulæ of the logician. The reason for this quarrel between two honorable branches of science is simply, of course, that the psychologist has the inveterate habit of including in his term reasoning the search for what I have called (a technical term) the "adequate" premises—which is half the battle, to be sure, when one is engaged in thinking out a solution to real difficulties. The pure logician, on the other hand, cares nothing for this aspect of the matter—he is concerned only with the validity of structures of premises. I propose to use the term *practical logic*, in a technical sense, for the psychologist's logic, and to call that of the logician theoretical logic, or pure logic. This simple device of giving two names to two different things ought to have the effect of modifying the contemptuous terms in which the psychologists sometimes discuss the logicians.

Why should not the scientist, whose constant occupation is practical reasoning, devote some time, now and then, to polishing up the tools of his trade? Why should he not make a special study, when occasion offers, of the great quagmires of bad reasoning that, in various fields, lie behind him? And it is exactly the psychologist who will have the best material for this study. It is safe to say that there has never been a subject of scientific research that has offered such a good field for studies of this kind as does the subject of color sensation. Both the old theories and the new—both the current theories and the non-current—are rich in not

only common errors of logic but far more, of course, in sins against the fundamental principles (axioms, as G. E. Müller calls them) of the neuro-psychic correlation. And as it is instances of bad grammar that we choose in order to teach the avoidance of errors of speech, so it is the mistaken theories of color which have high pedagogical value in sharpening up the wits of the intending reasoner. As matter of fact, moreover, it is some of the latest of the so-called theories which are quite the best pedagogically; they will in general have been devised with knowledge of the most recent ideas in physics, physical chemistry and physiology—all indispensable to the knowledge of color though not sufficient—and their errors in those plainest of methodological considerations which govern the making of theories in general and of psychological theories in particular will be found to be all the more curious and striking.

As a general principle, whoever notices some one of the innumerable effects of physical light that has not yet been made the basis of a color theory immediately utilizes it as such, quite ignoring the basic anomaly of color sensation—the existence of the "vanishing" color pairs—the fact that of the six possible dual color blends, the blue-greens, etc., there are two, the yellow-blues and the red-greens, which have never been experienced, and that in consequence (so different from what we have in taste) no tertiary blends and no quaternary blend have ever been sensed.

Theories which do not involve some hypothesis for the explanation of this curious circumstance are from the beginning negligible, and of this character are unfortunately most of the color theories of which English physical science has lately been so prolific.[1] Another class of negligible color theories is that of the authors who fail to have noticed that while red and green are (like yellow and blue) a disappearing color pair, they are not white-constitutive but yellow-constitutive; this may be, from the point of view of logico-æsthetics,

[1] In fact a table of the basic facts of color (such as Professor Warren gives in his 'Human Psychology') should always be kept before one when he sits down to devise a theory of color.

a misfortune, but it is nevertheless a fact, and there are many theories that are vitiated by ignoring it. The theory of Helmholtz falls, of course, into the first of these categories, that of Hering into the second. The latest of theories—that of Fröhlich, whose brilliant discovery of the oscillatory character of the nerve impulse in the optic nerve of the Cephalopod led one to expect much from an announced color-sensation theory—suffers, at the beginning, from the defect that it adopts the erroneous view of Hering that red and green are complementary (white-constitutive) colors.

As, in making a study of the proposed new international auxiliary language (Hilfsprache) Ido, you will have a keen intellectual pleasure in seeing how apparently insuperable difficulties in the way of simplification have been overcome by the 'Delegation' of clever language-logicians appointed by the International Congress of Philosophy of 1900, so in studying the wreck of color-theories which have received, and are still receiving, solemn consideration at the hands of the critic, you will have a keen intellectual pleasure in noting what remarkable vagaries the mind of scientific man has now and then been capable of. For the lectures on Logic which I am permitted to give at Columbia University, I find good store of illustrative material from the content of my lectures on theories of color-sensation.

Before taking up a discussion of the logic of color, I must remind my reader of the very essential reforms in color terminology which I have been urgently recommending since 1913, and which the Optical Society of America seems now to be taking up. Half the battle, in taking in the ideas regarding color which are necessitated by modern knowledge of color —neither Helmholtz nor Hering knew the truth regarding, for instance, the development of the color sense—consists in expressing those facts not in the primitive language of an earlier day but in precise, scientific terms

1. Not only do I insist upon regularly using the terms chromatic and achromatic for the specific or toned (as the Germans say) color-sensations, and for the non-specific, tone-

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less ones, respectively—most writers, following Wundt, do this already; but I also have boldly proposed the use of the shorter expressions chroma, achroma, in the corresponding senses.[1] It follows that the vision of the totally chroma-blind is not monochromatic but achromatic. The physicist should speak of homogeneous, not monochromatic, light rays, when they are (as they nearly always are) a dual color blend, blue-green, etc. An exact monochromatic yellow may happen on the other hand to come from the mixture of a troop of very non-homogeneous lights.

From chroma and achroma follow at once, of course, chromaticity and achromaticity, which are far better words then saturation and non-saturation. Saturation with what? A blue-green might be saturated with blueness or with greenness; other things can be saturated with sweetness; the word lacks the specific meanings that are conveyed by chromaticity and achromaticity. As, in the case of a series of blue-greens, we speak of the degree of blueness and of the degree of greenness, so here we express the degree of chroma and of achroma by the terms chromaticity and achromaticity. The free use of the term chroma will leave that terribly ambiguous word *color* to be used in its inclusive sense. To the age-old question, then, "Are black and white colors?" the answer is "Yes."

It is hard to fully disabuse the mind of the idea that external objects are colored. Color arises only after specific light-rays have effected their alchemy of the retina (and not always then). Instead of red, yellow, etc., when one means the specific light-ray combinations, one should call them erythrogenic, xanthogenic, chlorogenic, cyanogenic and leucogenic. How could chemistry have ever reached its present stage of development if chemists had balked at hard names?

The terms unitary color and color-blend explain themselves, and they are indispensable. A unitary red is the same thing as a red which is a psychological element, but that a given red is neither bluish nor greenish is the primary,

[1] The plural of *chroma* is of course *chromata*, save for such individuals as are in the habit of saying *scotomas* for *scotomata*.

direct deliverance of consciousness—that it is an *element* contains the addition of theory. Both terms are needed, but unitary comes first. Max Meyer uses the words singular color and dual color—also good and significant terms. But it is only of purely *chromatic* blends that we can say that they are dual only—a gray-green-blue is a quadruple *color*-blend. To the age-old question, then, "How many different chromatic sensations are there in the rainbow (or in the world)?" the answer is, "Four." The fatal confusion involved in the ineradicably wrong use (since Hering) of the terms luminosity and brightness I shall discuss at another time. The lately proposed term "brilliance" merely makes matters worse.

The universe in which we find ourselves consists of three regions—the psychical, the intracorporeal and the extracorporeal. Color phenomena, like other phenomena of the conscious organism, include in the first place the physical, in the last place the psychical, and in the middle place the physiological. For the two 'synapses' (if we may use this word in a metaphorical sense) which constitute the transition points between, respectively, these three regions, we need two names, and we shall also have need occasionally for a term expressing the final, non-direct connection between the extracorporeal-physical and the psychical. I suggest, in the interest of precision, the use of the term psychophysical parallelism for this latter sense (better still would be perhaps physico-psychic parallelism) in which 'physical' is allowed to have the technical sense of extracorporeal physical.[1] For the sense in which psychophysical parallelism is usually used, and for which many writers have already substituted the better term psychophysiological parallelism, I propose to say, since it is in fact only the nerve processes (and chiefly the cortical nerve processes) that are concerned, the neuro-psychic correlation. (This is the seat of Professor Warren's Double Aspect theory.) I shall then have the word physico-physiological for the connection between, *e.g.*,

[1] Wundt explicitly uses the term in this sense.

the physical light-rays and the physiological (in the first place the retinal) processes which they excite. This restricted use of the term physical has perfectly good precedent; we do not think of the 'physicist' as being engaged in the study of physiology, as a whole—in spite of the fact that the dividing line between physics and some parts of physiology has become very narrow.

I am now able to point out that the psychophysical 'parallelism' in conscious beings is a very different thing in the different sense regions. It holds, for instance, in the sense of taste; for the different taste-qualia we have chemical substances which are capable of stimulating the different sense organs on the tongue. What could be simpler? No 'theory' of the taste-sense is required, or has ever been proposed. For the temperature sense the situation is the same—there are different sense organs for the different sensations, heat and cold. In sound also the physical stimulus—an ordered series of vibrations of different frequencies—runs parallel to an ordered series of tones of different pitch. There are no mixtures of these frequencies which give you a *vanishing* sensation—all possible mixtures are recognized as tonal blends.

But the philosophers quite forget the subject of color when they virtually regard it as coming also under the head of a psychophysical parallelism (in the strict sense of the term). Note what becomes of the parallelism here. A single sensation, say a gray-green-blue, can be excited by a thousand different combinations of electro-magnetic vibratoins—by a million, rather—and certain pairs of vibrations (what ought to give you, for instance, a blue-yellow) vanish, as such, and give you a whiteness instead. It is as if, when you struck a *d* and a *g* on the piano together, you heard neither of these notes, but got a sensation of plain non-specific noise instead. This is what might be called a psychophysical perpendicularity, or topsy-turvyness, rather than a psychophysical parallelism. What to do? It remains for the physiologist (but especially the student of photo-chemistry) to step in and to find out what can be done in the way

of bridging this sad chasm. As matter of fact, our knowledge of this whole situation is such, at the present time, that, with the aid of the one simple hypothesis which forms the basis of my theory—that of a light-sensitive substance which undergoes development from a simpler form to a more complex form—we can make the whole situation fairly comprehensible. And the evolution of a chemical substance is not a far-fetched conception; we have, in the very organs which contain this light-sensitive substance, an instance of structural evolution. That the cones are more highly developed rods was a constituent part of my theory from the beginning:[1] it has since been turned from hypothesis to fact by the work of Ramón y Cajal. It may be mentioned in passing that there is no occasion for speculating about the significance of *rod-shape* and *cone-shape* (as does Mr. Troland, at some length; for it has been made out by Pütter that the one *difference-character* between them that has been preserved throughout the animal kingdom (since these visual elements came in) is simply the character of the connection with the second neuron—the rods have a knob-like ending, the cones have a dendritic ending. I said ('Dictionary of Philosophy and Psychology,' 1907, Art.Vision, p. 767) that these organs ought not to be called rods and cones (the cones are not even cone-shaped in the fovea), but that they should rather be named from their chief characteristics—knob-ended and finger-ended organs. This prevision of mine has now (v. Graefe, 'Handbuch der Augenheilkunde') been confirmed by Professor Pütter. The greater specificity of connection in the cones, of course, suggests a greater specificity in the character of the nerve-impulse, but until the neurologists have finally told us whether the fibrillæ in a single nerve fiber are capable of specific conduction or not, one cannot say whether this has significance. There is another thing that should be mentioned here: the synapses between the bipolar cell and the third neuron occur in five perfectly distinct levels (Luciani); this naturally

[1] Mr. Troland proposes to 'supplement' my theory 'by the assumption of an independent rod response for twilight vision.' 'The Enigma of Color Vision,' p. 13, 1921.

suggests a provision for the specific conduction of the impulses which end in the five specific sense-*qualia*—the four chromata and the whiteness sensation. (Blackness—a non-light sensation—cannot be connected with anything conveyed up from the retina, on account of the fact that it is of one intensity only.)

When the physiologist has finally done his work (which of course he cannot do unless he keeps combined in his one head the wisdom also of the physicist and of the psychologist), there remains the final transformation—that from cortical or possibly subcortical (Herrick) process to conscious experience, the neuro-psychic correlation. Here the correlation must be exact. It will not do to say with v. Kries (the most philosophical of the physiologists) "wir sind einem *gewissen* Parallelismus verpflichtet." It is not necessary (as G. E. Müller has pointed out) that every cortical process that can have a conscious correlate should *always* have its conscious correlate. Consciousness is a lambent flame, and it does not always follow upon the appropriate stimulation. But in the other direction the axiom must always hold. No two final processes of a *different* kind can both give rise to the same gray-green-blue. The distinction cannot be topographical, for, just as in the retina, the *topographical* spread-out-ness has to be utilized for the place-coefficients of vision. But the peripheral rod-whiteness and the foveal cone-whiteness (since they are of the same quality) cannot be due to *different* final processes. v. Kries says that I do not explain why rod-whiteness and cone-whiteness look alike. But in this he overlooks one of the main characteristics of my theory—not only do I explain this, but it is one of the principal things that my theory exists for the sake of explaining, as Hering has explicitly recognized.

The theories of Helmholtz and of Hering have been so fully discussed by the psychologists that their several inadequacies are now well known. Mr. Troland says (*Am. Jour. Physiol.*, Am. Optical Company, pp. 8–9, 1913): "The scientific value of the Helmholtz theory is at the present time almost negligible; it explains only the most rudimentary

of the phenomena of visual sensation; and that of Hering, in its pseudochemical, physiological, and psychological aspects is guilty of all manner of offence against fact and reason." Bayliss makes short work of Hering: the disappearance of the two vanishing color pairs cannot be accounted for by processes of assimilation and dissimilation, for there is nothing to prevent those processes from going on in one test-tube (or cone) at the same time. This is an argument that I have myself used against the Hering conception for a long time ('Professor Müller's Theory of the Light-Sense,' PSYCHOL. REV., 6, 1899). Parsons says that I am very severe on the theories of Helmholtz and Hering: I can only say in reply, in the words of a clever medical article that I have just been reading, "If I could be more severe, I would be."

But as I have already said, it is some of the most recent theories of color that present the most instructive instances of sins against fundamental methodological principles. Of particular interest in this way are the color theories of Mr. Troland. These theories are not, indeed, important in themselves, but they are, as I have said, immensely interesting from the pedagogical point of view, as showing what vagaries the mind of a clever scientist is capable of when it goes a-color-theorizing.

It is well known that Mr. Troland has a wonderfully fertile mind in the construction of theories;—there is hardly a subject of scientific investigation in regard to which he has not a theory of his own, and on some subjects he has more than one theory. His original theory of nerve-conduction (that of 1913) will be found reproduced at length in *Physiological Reviews* of last year. But his present theory of nerve conduction is a very different one—in it he agrees with the views of Professor Lillie, as those views were developed up to 1911. This later theory (what Mr. Troland calls 'our theory') is very fully set forth in the PSYCHOLOGICAL REVIEW of last year.

The earlier theory (of 1913) I find of very great interest from the pedagogical (and methodological) point of view.

In this theory of 1913 (what he calls a 'definite' chemical hypothesis) he follows the lines of Hering—there are assumed four distinct chemical processes to account for the four distinct chromatic sensations and a fifth to account for the sensation of white. (Black is, as in my theory—and in all theories except that of Hering—a sensation attached to a non-stimulated condition of the cortex.) But these processes are not antagonistic, nor reversible, as in the theories of Hering and G. E. Müller, respectively. This is, so far, an improvement over those theories; but it still remains to account for the vanishing of the vanishing color-pairs, red-green and yellow-blue. This mysterious phenomenon he explains in the following manner. The specific positive ions for yellow and blue, for instance, proceed safely through two synapses and through one bi-polar cell, but when they reach the large ganglion cell of the inner stratum of the retina [of the third neuron] a sudden exchange of partners takes place—a complicated molecule stands ready to yield up two specific negative ions to these two positive wanderers, and thus to prevent their proceeding farther. But their place is taken, obligingly, by the nucleus of this waiting molecule, which turns out to be a (double) *whiteness*-ion (and in the case of the red-greens a double yellowness-ion); in this way the white-sensation and the yellow-sensation take the place respectively of the missing yellow-blue and red-green.

I have tried to convince Mr. Troland (as he mentions in 'The Enigma of Color Vision,' pp. 28–30) that this conception is too much of the nature of a vicious *ad hoc* hypothesis—and even worse; for when once the four specific chromatic nerve-impulses are safely started on their way to the cortex, why should Nature, for no reason, interpose a process for effecting their mutual extinction? Our blindness to the red-greens and the yellow-blues is a loss, a defect (all blindness is a defect), and it can only have occurred as an unavoidable insufficiency in the method employed to make vision specific (as the heat-sense has not yet been

made specific—we cannot tell one wave-length from another in heat). It is true that Mr. Troland at first proposed the idea that this vanishing of the color-pairs might have sexual significance—that yellow and blue were colors of approach and retreat respectively, and that, in order to prevent the embarrassment, on the part of ardent youth and maid, of being tempted to retreat and approach at the same time, they were stricken with blindness (or their low-down ancestors were) to the yellow-blues and to the red-greens. But I understand that its author has at present given up this part of the first theory. My contention that his hypothesis is a purely *ad hoc* one Mr. Troland discusses, but does not assent to it and he still apparently defends this color theory.

In his second theory (*J. Opt. Soc. Am.*, 1917) Mr. Troland has ceased to feel the necessity for having yellow (or white) in his list of colors, and he adopts the three resonance curves of Helmholtz, hitherto overlooked by him. He actually says, in this paper, in plain black and white: "To explain the facts of color vision [by vision he means sensation] we have only to assume the existence in the retinal receptors for color (the cones) of three substances of this sort, having the maxima of their resonance curves in three representative regions of the spectrum, and to suppose that the relative intensities[!] of these three excitations, for any given wave-length, can be transmitted along the nerve to the brain." This would seem to be merely a momentary inadvertence on the part of Mr. Troland; he knows as well as do the psychologists that color sensation is tetrachromatic—that yellow (and white as well) needs to be accounted for in whatever purports to be a theory of color sensation. However, he repeats this statement in 1921. He says ('Enigma of Color Vision,' p. 19): It must be that "nerve-conduction must involve at least three independent variables, *and that three are sufficient*"—ignoring again the necessity of providing for yellow and for white. Fundamentally different as these two theories are, Mr. Troland usually refers to them as if he still believed in them both.

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The third theory of Mr. Troland is indicated briefly in his paper of 1921 (*Am. J. Physiol. Optics*, p. 18). The habitat of this theory is now that "advanced" region, the cerebral cortex. The vanishing of the color pairs becomes again, for him, antagonistic—namely a process of inhibition and excitation, and the 'excess of nerve energy' that is thus set free produces 'some' effect which is the basis of yellowness and of whiteness. However I fear that even this hypothesis, vague as it is, is not perfectly satisfactory to Mr. Troland, for he says at the end of this same article that the facts of color are still 'in a fluid condition' in his mind.

But the principles of logic can be usefully studied not only in the composition of new theories but also in the nature of the criticisms made upon theories that already exist.

Others have occasionally not fully understood what it is that my theory accomplishes, but no one has so completely misunderstood it as has Mr. Troland. The three important facts in color (I cannot too often insist upon it) are (1) the development of the color sense (with yellow and blue the middle stage, then red and green added), (2) complementation (but with yellow and blue *white*-constitutive, while red and green are *yellow*-constitutive), (3) the fact (Helmholtz, Young) that three initial *receptor-processes* are sufficient to start up the whole color gamut—that yellow and white come in of themselves—that they are secondary products—that no *new* light-stimulus is required to produce them. But what ones of the chromatogenic light rays are these three? Strange to say, *yellow* (which plays such an important rôle in the *development* of the colors) is left out,—all that are needed are red, and green and blue! This is indeed a mysterious situation, and the task of reducing to order and harmony such a contradictory collection of facts would appear to be (I am quoting from Mr. Troland) 'a tremendous one'—'making more rigorous demands upon the intellect than the formulation of far more cosmic theories.' But the problem of reducing these strange facts to harmony

and order I have undertaken to solve,[1] and by the simplest of assumptions. I assume merely that there has been a *development* of a light-sensitive substance, of this nature,—that it has become, by a new segregation of its molecules, by the formation of new constituent radicals, more *specific* in its reactions to light; but at the same time, since the molecules concerned are the same as before, certain of the decomposition products, when present together, unite chemically and form one of the previous nerve-excitant substances. This may of course not be the real state of things; it is offered as hypothesis (not as 'reality'), but it has perfect *vraisemblance*, and it affords a perfect explanation at once of complementation and of development. Until something better has been done, I hold that it invites consideration.

But it is exactly this fact—the explanation of complementation—that Mr. Troland has failed to notice in my theory. He says ('Enigma,' 15.2); "They are supposed to generate an *entirely new type* of nerve excitation which ends in a *yellow* sensation." Not new at all—this is just the point—my theory explains why they produce the *old* nerve excitation—the very one that caused the sensation yellow in the more primitive forms of vision. That is to say, I explain (among other things) why the *primitive* yellow is the *same sensation* as the yellow which (in tetrachromatic times) is made out of red and green, and also (what v. Kries too has overlooked) why the *primitive* white (in the rods) is the *same sensation* as the later white, that of the cones,—in spite of the fact that the cones are such extremely different organs from the primitive rods. (Hering has expressed appreciation of this feature of my theory.) It seems to me that this is accomplishing a good deal in the way of explanation, and by the simplest of means. Unless defects in this hypothesis can be pointed out, I conclude that it ought to be regarded (as it is regarded by many) as an efficient hypothesis. Mr. Troland says that it is ingenious, and that the basic outlines are probably essential (that is,

[1] The other facts of vision, after these fundamental ones are disposed of, are of course more easily handled.

indispensable) to any elucidation of the visual phenomena. (He means the light-sensation phenomena.) But this is exactly what I have myself said regarding my theory from the first—that it is, at least, a 'scaffolding' within which any final theory, when more detailed knowledge regarding the effects of light on the retina has been acquired, must be contained. Prof. Burton Sanderson (presidential address, British Association, 1893) after discussing the then recent facts regarding color said that they could be best understood in terms of my theory, and added, quite correctly, that my schema is, of course, diagrammatic. At the same time I hold, with Mr. Troland, that it is indispensable.

Mr. Troland's attitude towards my theory[1] is, on the whole, rather complicated. These are the features of it which he appreciates:

1. He accepts my theory of the black sensation (but he has not noticed my theory of why we have a sensation of blackness nor my proof that the blackness sensation is of one intensity only—that the series of grays are correlated with variations simply in the intensity of the white-constituent).

2. He yields to my insistence upon the fact that red and green lights, when mixed, are productive of yellow, not of white. (This is of course in complete contradiction to his Theories I. and II.—those of 1913 and 1917.) This view of mine has also been accepted by Westphal, by v. Kries and others, and it has virtually been accepted by Titchener, when he, with great honesty, plainly says that the Hering green and red are not the exact green and red, but verdigris and crimson. There are two horns, you will note, to this dilemma. The real red and green lights, when mixed, do not make white but yellow. On the other hand, if you take a sufficiently bluish red and a sufficiently bluish green ('crimson' and 'verdigris') you will have complementary (or white-constitutive) colors, it is true, but you will be dealing

[1] Good diagrams of my schemata—better in fact than I have been using myself—will be found in Woodworth's 'Psychology.' See my article in a forthcoming issue of *Science*.

with three (not two) unitary sensations, red, green and blue (we have here the great Helmholtz fact), and you will need to assume for them, on the most elementary principle of methodology, not two but three photochemical processes. My theory, in fact, is the only one that both takes account of this fact and offers an explanation of it. The Helmholtz theory so called accepts indeed the fact, but (being, as Professor Cattell has pointed out, both pre-psychological and pre-evolutionary) it has not felt under compulsion to offer any explanation of it. This, as I have said, is an element of my theory which Mr. Troland explicitly appreciates; it is in contradiction to his Theory I. and it is unexplained in his Theory II., but he takes it over into his Theory III.

3. He recognizes—the central feature of my theory—that it takes in both the sets of facts upon which the theories of Helmholtz and of Hering are founded (facts which are, respectively, subversive each of the other theory). He says, after 'supplementing' my theory by the assumption of a primitive white process in the rods (which was of course a fundamental and essential part of it from the beginning), "I think it is clear that the Ladd-Franklin theory is distinctly superior to the other two. This theory establishes contact on the one hand with the [four] psychologically primary color qualities and on the other hand it takes cognizance of the three-color-mixture laws upon which the Young-Helmholtz theory is based." ('Enigma of Color Vision,' Am. Optical Company, pp. 13–14.)

Mr. Troland's explicit objections to my color theory are two in number:

(1) He thinks that any theory of color should deal exclusively with cortical processes—he says that retinal processes are probably 'negligible' as throwing light upon these phenomena, and that the more 'advanced' view is that the basis for the peculiarities of color is to be found at some higher level. (It sounds a little like Christian Science, and New Thought, to say that some views on colors are more 'advanced' than others. Topographically it is true that the cortex is more 'advanced' than the retina.)

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(2) His other objection is that in order for the partial dissociations to take place that my theory requires, there must have existed in the original whiteness molecule such aggregates of atoms as are those now to be broken off.

I take up the first objection first. In this tangled course of events from physical light to psychical light-sensation—from a 'rectilinear' series (to use G. E. Müller's good term) of electro-magnetic vibrations to a poverty-stricken four chromatic sensations with their four (not six even, as there would naturally be) series of color-blends, it is absurd to say that any *one* event is of 'prime importance.' It is a total misconception of fundamental principles of methodology not to recognize that if we have a causal chain of events such that *A* conditions *B*, *B* conditions *C*, *C* conditions *D*, etc., each one of the intermediate events is, not only of prime, but of absolute importance. Who does not know that a chain is no stronger than its weakest link?—though there has not often been occasion, perhaps, to set it down as an axiom of scientific logic.

What Mr. Troland really means to say is that in his opinion the specificity of the visual chain of processes is more likely to reside in the cortical processes than in the retinal ones. But this is merely an opinion, and the fact that we know more about synapses than about photochemistry (if it is a fact) does nothing (though Mr. Troland seems to think it does) to support it. I agree with the opinion of Dr. Howell (just expressed to me in a personal communication) that the specific features of the visual chain of events are more likely to reside in the receptors, and in the receptor processes, than in any of the nerve-structures. When once the nerve impulse has started on its course it would seem to be practically the same thing in every sense region (save for the fact that the psychologist may demand of the physiologist provision for at least five different mechanisms of conduction in a single nerve fiber). In the debouching of nerve impulse upon that cortical structure, whatever it may be, which is correlated with the psychic experience of redness or greenness, there must indeed be some-

thing specific. There is no reason to think however that a 'red' excitation and a 'green' excitation can proceed uninjured to the cortex only there to be degraded into the basis for a more primitive yellow sensation.[1] It is far simpler to suppose that the 'transformer mechanism' (by which for innumerable wave-length combinations are substituted *five* colors only) has its site in the retina. But Mr. Troland says (p. 15) that my theory would be more satisfactory if it were transferred to the cortex. I have no objection to carrying on, at need, a second theory in the cortex, but I confess that I still prefer the retina as my theory's principal habitat.

When it comes to the final link in the chain—what I have called the neuro-psychic correlation—we have indeed something which, as Mr. Troland says, is the most 'interesting' stage of all. But it is a wilful ignoring of fundamental principles to suppose that light-sensation (or anything else) will ever do anything towards 'explaining' it. What is an explanation? It is a subsuming of facts, or of partial laws, under some more general law of which they constitute particular instances. But this connection of final cortical process with conscious experience is unique—*sui generis*—there is no companion-piece to it in the universe elsewhere, and there is no more general situation under which it can be subsumed; hence the effort to explain it will be futile. Its attendant conditions we may know more about, but the real *why* of the situation is plainly undiscoverable.

Mr. Troland's second objection is not only trivial, it is also purely factitious. It is that if there are three groups of atoms in my final molecule which are separately cleavable by light, they must have existed in it from the beginning. This is not only a complete misunderstanding of my theory—it has also no foundation in fact. It is difficult to know where Mr. Troland got this idea, that if certain substances are separated out from a given mother substance they *must* have existed already preformed in it. This fictitious rule

[1] In the Theory I of Mr. Troland they only reach a large ganglion cell of the retina, there to be captured and to set free a waiting whiteness excitation. It may seem to some that this idea lacks 'reality,' and *raison d'être*, though it is true that it has 'definiteness.'

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does not hold even in the building of the complex nuclei of atoms out of the positive and negative electron groups of which they are composed. I have just come upon this statement by Professor Harkins who is doing such important work in the evolution of the atom (*Phil. Mag.*, *42*, 333, 1921): "That groups which are not contained as such in more complex groups may nevertheless be separated out from them is one of the common experiences of workers in chemistry; for example, anhydrous oxalic acid, with a formula

$$\begin{array}{l} \text{HOC—COH} \\ \quad\;\; \| \quad\; \| \\ \quad\;\; \text{O} \quad\; \text{O} \end{array}$$

which does not contain the group HOH, nevertheless gives water when it is heated."

In the case required in my theory it is not a matter of a simple chemical reaction, but of the development of a chemical substance in the interest of greater specificity to light radiations; it is an idea, therefore, which is still further removed from being questionable. In fact the belief that substances must be preformed, if they are to undergo cleavage, is, my chemical advisors tell me, a conception that has not been current in chemistry since 1845.

But is my idea of the evolution of a color molecule so out of the usual? I admit that when I first proposed my theory this plan of paralleling (1) the known psychical development of sensation (non-specific whiteness into tetrachromatism) and (2) the known structural development of the visual elements (rods into cones) by an assumed development in the photochemical substance of the retina was somewhat venturesome, though even at that time it can hardly have seemed far-fetched. But the situation is completely changed now. Mr. Troland, who takes this view, but who at the same time explicitly accepts my idea that the added red and green sensations of normal foveal vision are not of the nature of a simple addition (as he took them to be in his Theory I.) but that they constitute a real evolution out

of a more primitive yellow sense, has probably not noticed that the *evolution of chemical substances* is at present very much to the fore. Aside from the wonderful new theory of Perrin—the radiation theory of chemical activity—I take it that there is nothing so exciting at present going on in the scientific world as the making out of the evolution of the atom by Soddy, Rutherford, Bohr and Harkins. I find myself, in fact, in the best of company in my conception of a chemical evolution, and I consider that Mr. Troland is a little old-fashioned in opposing it.

But I have, as it happens, a very beautiful companion-piece to my own conception—of such fundamental importance, in fact, as to give me all the analogy that I can ask for. Chlorophyll and hæmoglobin (perhaps the two most important substances in nature, Bayliss says), are both constructed out of practically the same constituents—both are compounds (with other substances present also, namely iron in chlorophyll and magnesium in hæmoglobin) of three dimethyl ethyl pyrrols. Willstätter (in a personal communication to Bayliss) uses these very striking words,—he says that he does not regard the similarity in constitution between chlorophyll and hæmoglobin as being of any great significance. (By this he means that it is nothing extraordinarily unusual.) I quote from Bayliss: "The mother substances were probably at hand, and compounds with the properties of the two pigments, respectively, being required (if one may use the expression) these pyrrol derivatives were made use of."

With this analogy, so very much to the point, I confidently rest my case, as far as a chemical evolution is concerned.

I cannot then too strongly urge upon the scientist the study of practical logic as it is exhibited in the good, but more especially in the bad, theories of color. The subject of light-sensation is a difficult one in the sense that there is no parallel in any other domain of the senses to throw light upon it, and also that the facts, which are numerous,

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are, indeed, when not woven together into some consistent structure, complicated and confusing in the extreme. These complicated facts have given rise to many attempted solutions, most of them however of a negligible degree of *vraisemblance* and wholly lacking in conformity to the simplest methodological principles. It is probable, in fact, that no subject of human endeavor has ever been the source of more vagaries and misconceptions—not even the squaring of the circle or the trisection of the angle—than will be found in the existing theories of color sensation, sixty or seventy in number according to Professor Frank Allen.

I do not however agree with Mr. Troland when he says that the task of comprehending color is a 'tremendous' one, and that it makes, perhaps, 'more rigorous demands upon the intellect than does the formulation of far more cosmic theories.' If, indeed, one envisages the phenomena of color from the point of view of the way in which the color sense has actually been developed in the animal kingdom, and if one also bears in mind that instead of a spacious cochlea Nature had only a very minute space—an infinitesimally small cone, in the fovea—in which to set up a whole new mechanism for specificity, it will be found, I believe, that the phenomena of color are by no means so much more mysterious than other events which psycho-physiology has to deal with. Certainly when it comes to the nature of nerve conduction, and of the final neuro-psychic correlations, there is no reason to think that the domain of color differs fundamentally from any other of the sense domains. And in the light of the known course of development of the color sense, the facts of peripheral vision and of the illuminating progression of stages of color-blindness cease to offer difficulties.

A more explicit discussion of the logical principles that should govern the making of theories of color will be found in my paper 'The Theory of Color Theories.' [1] The science of methodology has of course a totally new problem pre-

[1] Comptes rendus du VI[e] Congrès international de Psychologie, Genève, 1909, 698–705.

sented to it when it reaches this subject. In other sense-regions variations in stimulation produce variations in consciousness, but that is far from being the case in color. In general, a point on the color area can be got by countless different combinations of wave-lengths. Such a sensational point is however uniquely expressed in terms of the three variable quantities which have been substituted, in the retina, for the octave of frequencies of the visible energy-radiations. This constitutes what I have called a "transformer mechanism"; if one fastens his mind upon the color-triangle (the map of possible color-sensations), and forgets the visible spectrum, comprehension of the phenomena of color will be greatly facilitated.[1] There is, it is true, an immense amount of work to be done (and being done) in making out, quantitatively, the details of each stage of the color-sensation processes, but nothing is gained by over-emphasis upon the fundamental difficulties of the subject.

[1] PSYCHOL. REV., 23, 242–245, 1916.

THE SIGNIFICANCE OF PSYCHICAL MONISM FOR PSYCHOLOGICAL THEORY

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The present period of controversy concerning the problem and scope of psychology appears to be characterized on the part of each of the disputing factions by an inability to perceive the facts and to conceive the ideas upon which the contentions of their opponents are based. This prevalent psychic blindness, however, is nowhere more clearly manifest than in the attitude of both introspectionist and behaviorist schools towards the doctrine known as psychical monism. This doctrine, which has appeared to the present writer since the very beginning of his studies in psychology to provide the simplest and most complete explanation of the psycho-physical relationship, has been expounded independently by several philosophers, or psychologists, since the time of Fechner [3],[1] but has apparently found a place in only one[2] out of the multitude of extant English textbooks of psychology. The cause of its failure to make an impression upon the psychological and philosophical public is not to be found in any lack of clearness in its conception or exposition on the part of its apostles. Clifford's [1] brief outline of the theory is a model of lucidity, while the elaborate discussions—by Strong [8] and by Heymans [3] leave scarcely any angle of the hypothesis unilluminated. Since a considerable group of the problems which constantly arise in modern psychological debate are reduced to objects of straw under the influence of this hypothesis, one might be tempted to attribute the failure of the theory to gain a wider acceptance to an intellectual incapacity on the part of contemporary psychological and philosophical thinkers.

[1] Numbers in brackets refer to the bibliography.

[2] Stout, G. F., Manual of Psychology, London, 1907.

However, if we wish to avoid this painful conclusion we may adopt the alternative of supposing that the advocates of psychical monism have failed in vociferousness; hence the present paper.

The essentials of the hypothesis in question are capable of being stated very simply and clearly, although the various conflicts which appear between its implications and the erroneous postulates of common sense soon lead to great confusion for the average thinker. For the psychical monist, consciousness, or the psychical, is identical with individual experience at any instant. *Introspective psychology* is merely the straightforward description by any individual of his own experience in its own terms without addition or subtraction from the given. No individual can describe in this manner any other individual's consciousness except in so far as the two consciousnesses overlap, which overlapping is assumed in fact never to occur. Physical science, including physiology, is distinguished from psychology both in regard to its method and its results. Now, physical science, although based upon individual immediate experience, is not concerned to obtain a mere description of such experience, but rather is seeking an account of certain supposed realities which lie outside of that closed experiential system and which are responsible for certain aspects of the latter. The doctrines of physics take the form of an *interpretation* rather than of a description of experience, this interpretation being couched exclusively in terms of concepts of mass, length and time (possibly with the addition of electric and magnetic dimensions) eliminating the myriads of other qualitative differentiæ which are actually found in experience. The entire system of physical science, including physiology, is, therefore, in reality a metaphysical or a *metempirical* theory; the modern physicist's conception of the universe as a complicated dynamic mosaic of positive and negative electrons cannot be regarded with any reasonableness whatsoever as a direct deliverance of experience. It is nevertheless the ultimate or fundamental thing in physics, and its hypo-

thetical character does not detract in the least from its stability in that realm. All biological and physiological concepts, including those of nervous operation or response which interest the psychologist, must ultimately be reduced to the centimeter-gram-second, electro-molecular constitution which is demanded by general physical theory. No matter how much the structures and processes of the nervous system may be integrated, they are ultimately nothing but a specific complicated and changing mosaic of electrical particles.

Up to fairly recent times it appeared possible for the physicist, if not for the critical philosopher, to regard the physical system as a description of something which *actually existed*, although it was not immediately found within any individual's experience. However, the developments of the relativity theory appear now quite to preclude this possibility. Formerly the physicist could believe that—although colors, tones, temperature qualities, etc., were merely psychological—space, mass and time were not only psychological but also externally real in nature. Relations which appear in Einstein's doctrine, however, make the nature of space, mass and time as dependent upon the observer as are the so-called secondary qualities. The physicist is still permitted to believe that his elaborate mathematical system is some sort of symbolic representation of an unknown external reality, but as to the qualitative nature of this reality he knows nothing. The path pursued by physics leads constantly away from the discovery of the intrinsic nature of the external universe.

In this situation, the psychical monist turns back to psychology, and in particular to physiological psychology, for an answer to the metaphysical question. He returns as a thinking individual to the classical laboratory psychological experiment in which he finds himself inferring in physical terms the operations of a certain nervous system, and at the same time believing that the words of the subject which he hears are actually indicative of the existence of

a certain system of psychological entities—the other man's consciousness—which is associated with the nervous system in question. The detailed examination of many sets of data has led him to the general conclusion that this type of association consists in a point-to-point correspondence between psychical and physical terms and relations, a correspondence ordinarily called parallelism. The subject's consciousness is, from the experimenter's point of view, without any doubt an objective reality, but the nervous system and physical organism of the subject are pure hypotheses, the organism which the experimenter sees or feels being obviously nothing but an ephemeral complication of visual and tactual superficies. In the situation what is more obvious than the idea that the external reality corresponding to the physical hypothesis is nothing at all other than the subject's actual field of experience, or consciousness? This is the essence of the psychical monistic theory: The world of external reality is simply a complex system of psychical entities which are generally similar in character to the internal system which comprises any given observing or thinking individual. The entire system of physics is merely an unfinished product of the efforts of such an individual to arrive at a formulation of the abstract relations existing between the components of this external system. The physical scheme, in other words, is *formally identical* with the external psychical scheme, but its terms or qualities are absolutely different from those of the latter.

Following the indications of classical psychophysical data, we must suppose that the introspective consciousness of any subject in the psychological experiment finds physical representation, not in the sum total of his response processes or even in the totality of his nervous activities, but only in a very limited central or synaptic process occurring in his cerebral cortex. Since this elaborately hypothecated cerebral synergy is merely the symbolic representation in physical terms of the subject's actual consciousness, there is every reason why the two should have a parallelistic

correspondence; but since the physical side of the correspondence is undoubtedly a fiction, the system is not dualistic. The only existential correspondence which is to be found is that which necessarily exists between the consciousness of the subject and the *effects* which this consciousness is producing in the second and separate consciousness of the experimenter.

The doctrine of psychical monism is immune to the attacks of Occam's razor, because it quite eliminates from the realm of reality one of the terms in the psychophysical dualism, *viz*, the physical term. It retains the abstract scheme of the physical system intact, but substitutes for space, mass and time (and electricity and magnetism) other terms of a more general psychical character. Space, mass and time are reduced to the level of specific psychological categories, thus falling from their exalted position as general dimensions of the universe. Space, mass and time are merely certain characteristics of perception in terms of which it has been found possible to work out a symbolic formulation of the system of external reality. But since the psychical monist still believes in an external universe equally complex with that which is pictured by the physicist, he must suppose that any introspective consciousness occupies a position in that general psychical system wholly analogous with that which is occupied by any focal, cortical, brain process in a complicated response activity. If the physical physiologist depicts a propagation of influences from stimulus through receptor and afferent path to the central synapses and out again efferently to effectors, then the psychical monist must suppose an entirely analogous series of processes taking place among organized psychical entities, and in which the introspective consciousness of any observer corresponds to the central, synaptic activities on the physical side. Utilizing a terminology introduced by the present writer, the introspective consciousness is the para-cerebrosis, but there must also be para-stimuli, para-receptor operations and para-motor results. The introspective system is thus an actually affected and effective term in a coherent dynamic or para-

dynamic mechanism, which mechanism however is not made of matter but of the variegated stuff which constitutes consciousness.

Is this too difficult a doctrine for psychologists to grasp? I trust not, since once it has been clearly envisaged, nearly all of the dilemmas which are worrying modern psychological theorists and the realistic philosophers, disappear like darkness in the light. The old bugbear of interactionism versus parallelism is laid away in its grave. Psychical monism explains how the relation of seeming parallelism can arise in a system which is throughout interactionistic. Each introspective field of consciousness in the psychical monist's universe exercises an influence upon the course of events in the universe in conformity with exactly the same laws by which any other factor in this universe exerts an influence. There is no conflict with the principle of the conservation of energy, because this latter principle is purely physical and in order to gain significance for the external universe of consciousness, it must be transformed into a corresponding psychical or paraphysical law. This latter law is the actually effective principle, the physical principle being merely a symbolic translation of it. Just as the influences exerted by afferent nerve currents upon cortical synaptic processes and by the latter upon efferent nerve currents are presumably quite in harmony with the principle of energy conservation, so the influences exerted upon the introspective consciousness and those which emanate from the latter will be quite consistent with the principle of conservation of para-energy in the external psychic universe.

The hypothesis of psychical monism explains at once why the introspective consciousness is apparently determined by variables which are localized in a highly restricted region of the nervous system, in all probability one of the association areas of the cerebral cortex, and why this introspective consciousness has only a secondary determinative relationship with afferent and efferent nerve currents. If the introspective consciousness is, as it appears to be, a

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coherent group of psychical entities which is acted upon from the outside by other agencies and which in turn influences such other external realities, it should correspond to that portion of the physiological response mechanism which most integrally represents the same factors, sensory and motor, which find representation in consciousness. This condition is apparently satisfied by the focal brain process alone. Many modern semi-behavioristic writers are inclined to believe that consciousness depends simultaneously upon more than one stage in the response process, thus not being tied up exclusively with the central activity as is supposed by the classical specific energy theory. Kempf,[1] for example, believes that the affective aspect of consciousness depends directly upon the functions of the autonomic nervous system, while the sensory aspects are determined by receptor processes, and these two aspects are welded together by the cortical activity. Dr. Head[2] believes that the affective consciousness depends upon thalamic processes directly while the discriminative aspects of sensation are determined by the cerebral cortex. Dr. Kantor,[3] Professor Holt[4] and others are making strenuous efforts to correlate all of the categories of consciousness with efferent operations. A careful analysis of extant data, however, in the writer's opinion, shows quite clearly that these views are attributable to the urge for novelty or for objectification rather than to demands of data. The actual results of psychophysical observations favor what the present writer has called the *monophasic cerebral theory* of the determination of the introspective consciousness, and the laws which actually connect afferent or efferent processes with consciousness are explained in an entirely adequate way through the medium of purely physiological interrelations between central and peripheral factors in response.

While it cannot with fairness be claimed that the monophasic central theory has been absolutely proved by extant

[1] Kempf, E. J. The Autonomic Functions and the Personality, (New York) 1921.

[2] Head, Henry, 'Sensory Disturbances from Cerebral Lesions,' *Brain*, 34, 102–253.

[3] Kantor, J. R., 'Suggestions toward a Scientific Interpretation of Perception,' PSYCHOL. REV., 1920, 27, 191–216.

[4] Holt, E. B. The Freudian Wish (1916).

observations, it can be said without hesitation that this theory is the simplest explanation of all of the facts and that there are no facts as yet accessible which demand a polyphasic or a peripheral doctrine. On the whole, consciousness appears to be an afferent rather than an efferent product, so that the semi-behavioristic theories appear to be working quite in the wrong direction. This is not denying, of course, that under normal conditions the forms of consciousness are correlated with specific motor expressions, but this correlation appears to be merely an implication of the continuity of the normal response mechanism, the biological function of which in its entirety is, of course, to be found in action. For the psychical monist all of these correlations rest upon the fact that the physical scheme is merely an artificial, conceptual caricature of the introspective reality, the physical being thus an epiphenomenon of the psychical rather than *vice versa*.

From the point of view of psychical monism, the teachings of extreme behaviorists of the Watsonian type are outrageous in the extreme. The psychical monist would not for one moment deny the possibility of working out a physical explanation for any organic function, no matter how intricate. The physiologist does not need to introduce consciousness into his scheme at any point; as a physiologist, that is as a special kind of physicist, he has given up the study of consciousness at large and has restricted himself to the task of formulating the manifestations of the universe in physical terms. He must pick terms which will permit him to arrive at a closed system. From the standpoint of psychical monism the radical behaviorist, on the positive side of his activities, is simply a physiologist in a limited field. If behaviorism is the external study of the responses of organisms to stimuli, without reference to the detailed mechanism by which these responses are consummated, then behaviorism may be regarded as extra-cutaneous or superficial physiology. If, on the other hand, it is a study of the manner in which already complex physiological mechan-

isms are fitted together to yield a biologically useful total reaction, then it is simply physiology of a gross rather than of a refined sort. The psychical monist can have no quarrel with either of these forms of physiology, although he may well doubt their self-sufficiency, or the ability of their exponents to maintain them as distinct subdivisions of physical science. When, however, the behaviorist says that he does not know what consciousness is, or denies that it exists, his expression seems to the psychical monist to be tantamount to a confession of complete ignorance or to a denial of the basis of all knowledge. Consciousness, for the psychical monist, is simply experience, and if the teachings of modern philosophy have any fruits at all, they certainly lead us to believe that all knowledge is derived from experience. If behaviorism is an empirical science it is based upon the observation of what the psychical monist designates as consciousness. This consciousness cannot, therefore, be unreal. Introspective psychology is merely a study of this consciousness, or its temporal variations which constitute experience, in and for itself without hypothesis. Behaviorism is a highly restricted interpretation of certain very limited aspects of the consciousness in question, *viz.*, certain, mainly visual, perceptions which are only obtained under the circumstances of physiological observation.

The latent intellectual utility of psychical monism for the psychologist is clearly demonstrated in the demands which appear in certain recent discussions. We may take as an example an admirable article by Professor Warren,[1] with the general thesis of which the present writer is quite in agreement. Professor Warren insists upon the necessity of a monistic, or double-aspect, interpretation of the psychophysical nexus and yet appears to be quite unable to show how this monism is possible, since he admits that the phenomena of consciousness and those of the central neural processes, upon which he conceives consciousness to depend, are extremely different in character. He believes

[1] Warren, H. C., 'Psychology and the Central Nervous System,' PSYCHOL. REV., 1921, 28, 249-269.

that, in spite of this obvious discrepancy, it is still permissible—at least pragmatically—to identify them. To this belief the present writer is quite unable to subscribe, since it is clear to him that no two things which are in the slightest degree different in nature can be in the slightest degree identical. If consciousness and the brain process are one and the same thing, then one of the descriptions of this unitary being must be sadly in error, and clearly the only possibility of error lies in the physical system, since the introspective facts are immediately given, while the physical scheme is the result of a laborious and dangerous process of inference. Psychical monism announces blatantly that the entire physical scheme is grossly in error qualitatively, although it is quite correct in certain aspects of its mathematical form. This attitude permits a belief in monism which has no reservations and no unclearnesses. The physical scheme still exists in some sense, but only in the mind of the physicist when he happens to be thinking it, and the psychical monistic system at large makes it quite clear how he happens to think in this particular way when he does decide to think. There is actually—in Professor Warren's words—"a duplicate set of events" but one member of the set is of quite subsidiary and ephemeral importance in the universe as a whole. The psychical monistic theory, however, should interest Professor Warren in this connection since he says that the mutual relations of the members of this duplicate set of events "are not made clear in any statement with which [he] is familiar."

The present writer hopes in later papers to discuss in great detail many of the implications for philosophy and psychology of the psychical monistic, or—as he chooses to call it—the *paraphysical*, theory. In the meantime, however, he wishes to call attention of psychologists, and incidentally of philosophers, to the existence of this doctrine, which has already been expounded in great detail and with marvelous clearness by Heymans in his *Einführung in die Metaphysik*, as well as by a number of other writers.

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SYNÆSTHESIA, A FORM OF PERCEPTION

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This investigation aims to answer the following questions: (1) Are the secondary processes, so called, in the phenomenon of synæsthesia, incidental or essential in the mental life of the synæsthetic individual? (2) What is the behavior of attention between the two associated processes in any given example of synæsthesia?

The experimental method here pursued was as simple as possible and the results have been obtained throughout from introspective data. While only one case of synæsthesia has been available for this study it is believed that an intensive and thorough analysis of even one genuine case should throw some light upon the nature of the problems involved. Trained synæsthetic reagents are sufficiently rare to warrant the report of a single instance.

Our reagent was the junior author, a graduate student in psychology and an introspector of several years' experience. He lost his sight at the age of eleven and has noticed his synæsthesia ever since he has been able to remember, which was long prior to his blindness. A detailed analysis of his case, together with a history of the literature on synæsthesia, has been published elsewhere.[1]

In the experimentation the observer (*A*) was instructed to focus his attention upon some reading matter which was presented him. While he was thus engaged in reading, the experimenter stimulated him with various sounds, tactual and other stimuli. In a series of check experiments *A* was asked to anticipate the sound or the contact.

The following are a few examples of a vast amount of introspective data which have been obtained, always under the same conditions, over a period of two academic years.

[1] 'The Synæsthesia of a Blind Subject,' University of Oregon Publications, I, No. 5, 1920.

1. *Stimulus:* The tapping of a bell with instructions to *A* to anticipate the sound. "Tried to anticipate the sound in terms of leaning my head in the direction of the experimenter; called up detached visual images of familiar sounds; said 'sound' to myself several times; found myself totally unable to obtain auditory imagery of any sound for all I could obtain was a series of visualized sounds. When the bell was tapped the first experience I had was a visual image of a small, silvery cloud; could not possibly have said that it was an auditory stimulus until the visual image floated into my field of vision from the direction of the experimenter. All this happened very quickly, with exceedingly rapid shifts of attention. Then by means of eye-movement I shifted my attention vigorously in the direction of the stimulus. Then for an instant I thought I heard the sound itself as such but I noticed definitely that it was non-focal; the visual cloud momentarily vanished but far quicker than it takes to say it, it reappeared and I found that with the reappearance I was again aware of the sound. In attending to the sound I find myself focused upon visual rather than upon auditory qualities. I tried it again, while the sound was still lingering; this time I said 'sound' in vocal-motor fashion and had intensive strains in my neck of leaning toward the source of the stimulus. Here I thought I succeeded in attending to the sound itself, focally, but as before, discovered that what meant the sound to me was in reality this persisting, silvery, visual image. I was conscious of the metallic, mellow ring of the bell in terms of the silvery quality of the visual image; the intensity of the bell-sound was the brightness of the visual image; the changing quality and intensity of the bell came to me only in terms of changing quality and brightness of the silvery image. At no time was I getting away from visual qualities; they followed me no matter where I turned or what I did. The sound was present to consciousness but never with sufficient distinctness to describe. It was merely 'something' and was always a 'step behind' attention. I could tell you when the sound

stopped and when it began or when it changed, but always in terms of its visual associate."

2. *Stimulus:* prolonged blast upon a small, shrill whistle. *A* was instructed to do his best to attend to the sound qualities as such. "I find that I can attend to the sound as long as I do not attempt to analyze it; all the sound of which I am aware is an undifferentiated 'something'; it may be 'raw' sound; it is meaningless; I interpret the experience as sound in terms of the vocal-motor image, 'sound,' or my attention shifts to motor relaxation and I find myself merely responding to a 'sound' situation. I simply cannot focalize the sound as such in the absence of visual imagery; my alertness to the sound invariably appears in terms of its visual associate. All of the above I have interpreted from what actually happened. The instant the stimulus was given I became conscious of something; but this something was at once followed by a focusing of visual attention upon color and brightness. (Then a further attempt to shift from visual to auditory qualities resulted only in shifting from one visual quality to another.) As I tried, with a vocal-motor *aufgabe* to single out auditory qualities I found myself attending to single visual qualities; the intensity of the sound was present in terms of the sharpness and brightness of the visual image; a quality of the whiteness of the visual cloud meant the timbre of the sound; upon each attempt to attend to auditory qualities I found that the sound itself lingered in consciousness as an undifferentiated bit of awareness; the combination of sound plus visual image constituted the sound of a certain pitch, quality and intensity. I could attend to the sound itself as long as I was not listening to anything in particular and did not try to label the sound."

3. *Stimulus:* A piece of round, cold metal applied to *A*'s left temple while he was absorbed in reading. *A* jumped when the stimulus was applied. "Here the sensation of coldness appeared, visualized in color upon my temple, together with a widespread motor response of jumping. The motor reaction came before I consciously identified the sensation as coldness. At first I was conscious of something

hitting me; at this juncture I could not have said that it was cold; neither, at this time, was the sensation localized. Then the sensation became localized before it was felt as cold; this consisted of tendencies to visualize a halo of white light about my temple; the localization first appeared with the tendency for my line of regard to shift to that region of my head. Then, with the becoming conscious of whiteness as such came the meaning, 'coldness.' I subsequently noticed that my consciousness of something hitting me came in terms of a lingering awareness of my motor response."

(Further tests revealed a foreshortening in the procedure just described. Tactual sensations began to appear not as 'mere somethings' at the outset but 'something localized.' Here it was found that the visual process appeared with the original sensation and gave to it its localization. This foreshortening appeared as a rule when at the time of stimulation *A* was anticipating what was coming.)

Interpretation of Introspective Data

Introspections on acts of perceiving are extremely difficult. The mental processes take place with such rapidity that a tremendous amount of practice is necessary in order to cultivate an ability to note the processes just as they appear without unwittingly adding to them or leaving some important but only momentary shift of attention unnoticed. Further, it is impossible to separate interpretation from description of 'immediate content.' Description of immediate content is nothing but an interpretation reduced to its finest possible stage of analysis. Immediate content, as will be noticed from the above introspections, is something indescribable.

We find it necessary to distinguish between 'being conscious' and 'being conscious *of*' something. The first means that a mental activity is existing or taking place; the second means that two seriated mental activities are existing and that as a result of the temporal relations of these two, the

second labels or identifies the first. In other words, in the second instance, there exists a cognitive function—an act of recognizing or of interpreting that 'something' of which one is conscious.

A's so-called awareness of sounds, pressure sensations and other non-visual processes are not examples of being *conscious of;* but rather, they approach as nearly as possible the condition of actual 'raw' sensation—meaningless, unidentified, uninterpreted, 'raw' experiences. They are 'bits' of consciousness and that is all. They constitute awareness of 'something,' which in the absence of further mental contents practically amounts to an awareness of nothing describable. When *A* refers to these experiences he is forced to say: "There was an experience there but I cannot describe it," The instant he is able to say "I was conscious of a sound, something sharp, something localized, something blunt, something loud," etc., the experience which he previously described as merely 'something' has developed beyond the stage of sensation and into the stage of perception. This development in his case necessitates the use of visual synæsthetic images. The change from sensation to perception is brought about by means of the functioning of a stereotyped visual associate. As long as this visual associate fails to develop, or when, after long practice, the reagent is able to ignore the visual associate, a sound is neither a sound of an object nor of a quality; a pressure is neither the pressure of an object nor of a quality; a cold is neither a cold of an object nor of a quality. Without the visual associate, moreover, these sensations are not localized, but the instant the visual associate appears localization appears. Could there be more tangible evidence that synæsthesis in this case is not a freak phenomenon functionally but is *A*'s method of perceiving?

A's procedure in perceiving does not differ, functionally, from perceptions in the asynæsthetic individual. The only difference which we can find lies in the stereotyped nature of the associated imagery and in the fact that it is always

derived from the visual modality. The asynæsthetic individual apparently hears sounds directly, or feels contacts directly, because such associates as appear in connection with the original sensation blend with the latter while *A*'s do not.

The visual feature of *A*'s process of perceiving may enter consciousness previous to the original sensation feature. In such instances the dissociated visual image means nothing or symbolizes nothing until the primary process appears in non-focal consciousness or until the dissociated image leads to further visual imagery as the primary sensation would have done had it functioned instead of the detached synæsthetic image. Thus the detached synæsthetic image means nothing as long as it stands alone in consciousness or as long as it, alone, is attended to. Neither the primary nor the secondary process, then, can mean anything until it is 'identified' by its contextual associate or a proper substitute. Should the detached visual image appear in consciousness first, only the momentary presence of the sensory process in non-focal consciousness is necessary to give meaning to the visual accompaniment.

As far as the development of meaning is concerned, the two processes—the primary and the secondary—are reversible. Either one will 'identify' the other. The second to appear 'identifies' the first. The shift of attention from one to the other constitutes a process-aspect of the experience and is the essential feature in the act of perceiving. Without this shift of attention either the primary or the secondary process is detached from its associate and means nothing. By means of this shift of attention the mere condition of 'being conscious' develops to the condition of 'being conscious of' something. The cognitive function of mental activity so far as the act of perceiving is concerned, lies in part in this tendency for an antecedent process to merge into a subsequent process or for the second to issue from or develop from the first.

In general, then, *A*'s visual synæsthetic phenomena make it possible for him to become conscious of things. Without

this imagery his world would be a mass of undifferentiated and consciously meaningless jumbles of sensation until he found some other means by which he could identify this 'raw,' non-visual consciousness. It is his visual synæsthetic imagery which renders his mental life cognitive in function. Visual imagery for him is the basis of knowing. It is the basis of all forms of interpretation.

Without this visual imagery *A*'s mental life would undoubtedly lack the higher intellectual processes. He would be driven to interpret sensory experiences as we presume do the animals, namely, in terms of overt motor responses. Undoubtedly animals identify objects not in terms of conscious meanings, as we know them, but in terms of instinctive and habitual motor responses. The motor response issuing from a stimulus is meaning in animal behavior. The object is not consciously interpreted or analyzed. Otherwise the animal would possess cognitive functions, evidence of which is lacking.

While our experiments have been exceedingly simple, we believe that the results are far-reaching in their implications. For observer *A* sensations have no attributes—no quality, no intensity, no duration, no clearance, as such. Attributes are meanings. In themselves they are perceptions whose minimum simplicity consists of (1) an undifferentaited and indescribable experience and (2) a stereotyped synæsthetic image. There must be a shift of attention from the one and toward the other; the stereotyped image must behave in a sufficiently complex fashion to provide for analysis; there must be two features of the total experience, the process to identify and the identifying process.

Further, these results lead one to very seriously doubt that a 'one-level' consciousness can exist, or that a 'one-level' attention can exist. We note that *A* is able to report the existence of sensation in his mental life at all only by virtue of a shift of attention from it to the stereotyped process. When this shift is not present the existence of the sensation can be doubted. At least, in the nature of

things, the observer could never know that it existed. Evidence points to the fact that attention, like attributes, is a functional and not a structural phenomenon, whose minimum simplicity consists of a shift from one datum to another, the second of which must differ in some respect from the first. This means that attention is not 'clearness' but a phenomenon of meaning, *i.e.*, that form of distribution or change in distribution of mental contents which constitutes meaning.

Again, we find in this investigation that localization does not exist for *A* as a feature inherent in sensation, nor is it an unanalyzable phenomenon. It, again, is a product of change and growth, which, under the proper experimental conditions and with sufficient introspective experience, reduces itself to the behavior and functioning of synæsthetic images. We are led to believe, that, in general, localization is not a datum *sui generis*.

Finally, this and attending studies on synæsthesia point very definitely to the conclusion that there is no datum of consciousness which can rightfully be called 'unanalyzable' or 'sui generis.' The unanalyzability of our simpler experiences, is, without doubt, a product of syncopation, blending, attenuation. Had we analyzed more cases of synaesthesia, a few more cases of total amnesia and some very careful records of experiences by trained introspectors while going under and coming out from the influences of an anæsthetic, we would undoubtedly find that many so-called 'elemental' experiences are exceedingly complex in their nature.

Conclusions

1. Colored hearing and allied synæsthetic phenomena of peripheral origin are not, in our reagent's case, peculiar or abnormal mental activities. Rather, they are his normal and usual method of perceiving.

2. Without the secondary process, the stereotyped visual image, our reagent cannot perceive.

3. The sensory component, the primary process, is in-

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describable. The best that can be said concerning the sensory component is that it is 'something.'

4. These facts hold for every sense modality.

5. In our reagent's case attributes of 'elementary' experiences exist only when describable. For him sensations have no attributes. Such as appear to be attributes are data of consciousness only in terms of the behavior of visual synæsthetic images.

6. Attributes, so called, in our reagent's case, are meanings.

7. While the secondary processes in our reagent's synæsthetic phenomena appear to have attributes, evidence tends to show that these likewise are cognitive processes and not *sui generis* contents.

8. These results point to the conclusion that the simplest possible mental process, or at least the simplest describable mental process is a dual phenomenon implying an attention shift. Because of this attention shift, states, attributes and other structural features of mental life appear as interpretations and only as interpretations. They are arbitrary.

9. The theoretical considerations to which these facts ultimately lead are not new. Here we are confronted with the long famous difficulty of act and content, with the problem of 'immediate content' and other perplexing psychological difficulties. A continued discussion is reserved for a future paper.

10. If the results of this investigation are typical we may conclude that synæsthesis is a cognitive process, a phenomenon of meaning, and that the secondary image is essential. By means of its functioning, sensation develops to perception. Before this perception is complete there must be an attention shift from primary to secondary process or from secondary to primary. The sensory component and the image component are necessary but their order of appearance may vary.

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A COMPARISON OF MENTAL ABILITIES OF MIXED AND FULL BLOOD INDIANS ON A BASIS OF EDUCATION[1]

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There are two sorts of factors involved in all studies of mental process of the type about to be considered in this paper. One is that sort of factor which is inherent in the organism, and the other is that sort which plays on the organism and brings about changes in the organism, or the environmental factor. A division may be made of these two factors. In the first we find traits due to remote ancestry and those due to chance variation in less remote ancestry. In the second we have environmental influences of an informal character and those of a more or less deliberate or formal nature. Here we have at the lower end of the scale impressions whose moulding capacities habituate in the greatest minutiae, most informally, which shade very gradually into the more pretentious processes of formal education. Under this last, noting the cause of the change, we must take cognizance of the school of the mother's breast as a source of nurture as well as the school of the mother's knee, both of which profoundly influence the developing man mentally and physiologically in all probability.

And the school of the mother's breast and of the mother's knee are not the same for all individuals. They differ from group to group and from one individual to another but more so in the former case than in the latter. Especially would this be true if the groups were racial. The school of the mother's knee cannot be the same for a Navajo papoose

[1] The observations on social status among Indians included in this article were made in part while the writer was doing field work in the spring of 1921, the expense of which trip was met by courtesy of the Committee on Grants of the American Association for the Advancement of Science.

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living in its mother's hogan, reacting to its mother's bronze features and harsh guttural sounds as is that of the white infant, or the negro baby, in its early nest of qualitatively different stimuli. The patterns are different though they may be of the same elements, being richer in quality and more abundant in quantity in some situations than in others. The jewelled necklace, the silver bracelet with turquoise settings of the Pueblo mother, worn incessantly, and her brightness of costume generally will not give the same stimulus pattern and cannot produce the same reaction as a drab gingham dress with no beads and trinkets. We pass by the other differences in nurture which come later, but will say that teachers in some of the United States Indian schools where some boarders come in from the pueblos and the hogans at five years of age, claim that these children make better students than those who come later. It is easy to see that they have not so many Indian ways of thinking as those who linger longer in the primitive environment, which would interfere with the 'white' learning.

So when comparisons of mental performances of two races or even of social groups are made one must consider the environmental influence as a very important factor to be controlled.

We have said that individual environments differ in the same group and if they there produce differences of importance, racial environmental differences might produce them in larger degree. Here is a case, which so far as the writer knows has never been reported till now, of a white boy of eleven years with a Binet age of seven. The subject had been discovered in the typhoid ward of Bellevue Hospital of New York City and since his was an interesting case, upon his convalescence he was brought to the New York Clearing House for Mental Defectives for examination. He was given a Binet test by the writer, at that time a psychologist in the clinic, with the above-mentioned result. No pathological condition could be found by the neurologists in charge of the clinic, and until his life history was investigated no cause for the mental retardation could be made evident. But the

fact was that the boy had been reared on a coal barge which moved about in New York Harbor, never remaining for a long period of time at any one point. In this case the environmental influence had been so limited that he was retarded mentally four years, according to the Goddard Revision of Binet then in use, 1913.

Since certain of our tests presuppose certain environmental influences producing certain experience necessary to a creditable showing in the tests, to measure one by the test who has not had the opportunity to acquire the experience necessary before one can begin to meet the situation set down before him would be unfair. It is psychologically sound to say that one cannot learn something without the stimulus necessary to that learning. We cannot presuppose its presence. For that reason we cannot draw scientific conclusions as to the mentality of individuals unless those individuals have been exposed to the environment which the test presupposes. We may continually shut our eyes to this fact but the fact remains.

Negroes are compared with whites on a basis of age, likewise Indians with whites on this basis, but we do not know what the negro or the Indian could do if placed in an absolutely white environment from birth until the date of the test. This is no contention that the blacks and redskins as groups are necessarily equal to the whites, for we still leave the question open. So far tendencies appear to show that as groups they are not, where some allowance for environment is made. The value of experimental data cannot be unduly emphasized, being objective measures, but having obtained them they are only data after all. Analysis and interpretation of the most painstaking character are necessary to bring out all the facts since the distribution of the measures are merely resultants as it were of many factors.

However, when we study the 'comparative psychology of races,' the main factors whose influence we desire to determine are those due essentially to one given form of germplasm and another. We seek to find *race* differences and nothing else.

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We call attention to at least two ways of proceeding, both of which have difficulties. One of these procedures is to compare two remote but pure racial stocks in some mental performance. The other is to compare one pure stock with a mixture of the two racial bloods, *i.e.*, mixed bloods, as in the case of comparison of the performance of negroes with mulattoes by Ferguson (1).

The first procedure has the difficulty of controlling the factors of nurture, as environment, education, and social pressure. But if this could be accomplished the racial differences, if there be any, should be comparatively large. The second procedure ought to offer less difficulty on the score of controlling the environmental factor as the mixed bloods should possess an environment more like one of the pure blood group than if the groups were both pure. This method merely assumes less of difference in the matter of nurture. If there be any racial differences, they would be less in this case presumably, because one group is an 'adulterated' form of the other. But in this second method in which we compare pure and mixed bloods, if we are to draw inferences from the principles of the science of genetics, the matter is complicated by the dominance and recessiveness of traits found in such mixed bloods. To quote East and Jones (4), 'Inbreeding and Outbreeding,' pp. 55–56: "The interaction of a pair of homologous factors in a hybrid—allelomorphs they are called—does not always result in the production of an intermediate. Often the action of one factor *dominates* the action of the other, either by masking it or by inhibiting its operation. When this occurs the dominated character recedes from sight in the F_1 generation and the ratio in the F_2 generation is 3 dominants, 1 recessive." By way of illustration in the merely physiological character of skin color, a mulatto father with sandy beard married to an apparently full blood negress has two sons as black as their mother in striking contrast to their paternal parent. A little girl of three to be seen running about on a certain Indian reservation has a Jewish-Indian parentage, her father being an employee in the vocational school there, presents an interesting case. Will she show

equal amounts of Jewish mental traits and of Indian mental traits or more of one than of the other? If the genetic law works in mental traits as it does in physical traits possibly we ought to get multimodal effects in one distribution of mixed blood performance in a mental test, particularly should we get at least bimodal effects in a distribution of performance of an F_2 population. It would be going beyond the originally prescribed limits of this paper to follow this out further.

Beset by these foregoing difficulties in the present instance we have undertaken the comparison of full blood with mixed blood Indians in performance of some tests, taking advantage of whatever may accrue from the similarity of nurture in the two groups but recognizing that it is not a case of identity of environmental factors.

In a recent paper, 'The Results of Some Tests on Mixed and Full Blood Indians,' *Journal of Applied Psychology*, 1921, the writer of this paper gave results of a comparison of the performance of mixed and full blood Indians on a basis of age. The tests were the nine tests used by Pyle (**2**, **5**) on whites and negroes and some of them were used by Pintner (**6**). The data were so handled as to attempt to supply age norms for these blood groups, but because of the small groups of age and sex, resulting from these sub-divisions of the original 153 mixed bloods and 224 full bloods, the resulting 'norms' are rather unreliable. But the comparisons of scores almost consistently favor the mixed bloods though it should be said that the school attainment and other factors of nurture were ignored.

In the present paper we so handle the same data as to place both blood groups on an equal school attainment basis, using the Course of Study of the United States Indian School issued by the government as a basis of grading, which grading had already been done by the local administration, under the supervision of Superintendent C. W. Blair of the U. S. Indian School of Chilocco, Oklahoma, in which school the subjects were students.

The problem may read thus: If groups of mixed and full blood Indians with the same school training as indicated by

grade attainment are given the same psychological tests under controlled conditions, what will be the results of a comparison of their performances of the tests?

If the reader will examine the accompanying tables he will note the character of the tests administered to these subjects. It has been said (**8**) that if there are racial differences in mental traits they ought to be indicated by tests of the higher mental processes. We have here (1) association tests, controlled and uncontrolled which investigate, quoting Whipple (**10**), "the nature and efficiency of those association connections—that the subject has already established"; (2) memory tests which inquire into "the retentive capacity or ability to reproduce an arbitrary series of symbols" and "a series of related ideas"; (3) word-building tests, being tests "of invention—ingenuity and active attention—ability to combine isolated fragments into a whole."

The full blood subjects were nearly all of the Plains and Forest Indian racial stocks. Only two or three were Plateau Indians. The mixed bloods were mixtures of these stocks with whites, though a few appeared, by darkness of skin color, to have some negro blood; however, the experimenter could not establish this fact. Fortunately there were only three or four of these.

The size of the school-attainment groups which we shall call educational sub-groups makes the data fairly reliable with the exception of the eighth, ninth, and tenth grade sub-group of the full bloods. The writer has called attention elsewhere (**9**) to the fact that the degree of Indian blood decreases with school grade at least in this school—the higher the grade the smaller, on the average, is the amount of Indian blood.

The educational sub-groups are three in number and were gotten by combining the numbers in the fourth and fifth grades, the sixth and seventh grades, and the eighth, ninth and tenth grades respectively. In order to approach an even distribution of the number of cases these combinations were necessary and even then, in the case of the full blood sub-group mentioned above, the desired number could be brought

up to only forty-one as compared with ninety-four and eighty in the lower sub-groups, and in some of the tests the number was even smaller than this for this sub-group. We give herewith (Table I) the composition of the sub-groups in respect to the actual number taken from each grade as supplied from the data of the Opposites Test:

TABLE I

COMPOSITION OF SUB-GROUPS

Mixed Bloods			*Full Bloods*		
Sub-Group I	Number of Cases		Sub-Group I	Number of Cases	
4th Grade	14		4th Grade	37	
5th Grade	41		5th Grade	57	
Total		55	Total		94
Sub-Group II			Sub-Group II		
6th Grade	25		6th Grade	49	
7th Grade	28		7th Grade	31	
Total		53	Total		80
Sub-Group III			Sub-Group III		
8th Grade	16		8th Grade	17	
9th Grade	26		9th Grade	17	
10th Grade	15		10th Grade	7	
Total		57	Total		41
Grand Total		165	Grand Total		215

The numbers for the different tests are not the same, since the giving of the tests lasted over several days and sometimes subjects were absent from class. Sex differences are not measured here since the groups are all too small and will not stand further division and besides if such differences exist in the data they would probably be balanced by their equal distribution. As to age, grade for grade the average age is greater for the full bloods than for the mixed bloods by a median difference of 1.6 years or a difference on the average of 1.7 years with a range of 1.3 years in the fourth grade to 2.5 years in the ninth grade. If anything age should favor the full bloods in the results of the experiment for they are thus shown to be older, the difference between the averages being that of 11 per cent with the mixed group as a base.

We now look to the results of the performance of the two

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TABLE II

Sub-groups Grade	Opposites Test			Genus-Species Test		
	I 4–5	II 6–7	III 8, 9–10	I 4–5	II 6–7	III 8, 9–10
Full Bloods: No. Cases	96	83	40	114	85	38
Ave. Score	6.7	9.7	13.5	7.1	9.5	13.2
A.D.	3.0	2.9	3.0	2.0	2.2	3.5
P.E.	2.5	2.5	2.5	1.7	1.9	3.1
Median	6.4	9.5	14.1	6.1	8.9	12.7
Range	0–17	1–16	3–19	0–18	1–19	4–20
Mixed Bloods: No. Cases	48	53	46	46	43	57
Ave. Score	8.1	12.0	18	7.6	10.9	14.5
A.D.	2.8	2.7	3.7	2.9	3.2	3.4
P.E.	2.4	2.1	3.2	2.5	2.8	2.9
Median	8.0	11.8	14.8	6.7	10.0	14.3
Range	0–17	2–20	6–20	2–17	3–20	6–20
Per cent attaining median of Full Bloods	69	73	75	56	72	65

	Part-Whole Test			Free Association Test		
Full Bloods: No. Cases	85	77	39	105	81	38
Ave. Score	5.1	8.4	10.3	39	44.2	53.3
A.D.	.1	2.9	2.5	12.3	8.7	8.0
P.E.	.1	2.5	2.1	10.3	7.2	6.7
Median	4.9	8.1	10.1	40	45.3	51.1
Range	0–13	2–20	2–19	0–75	5–68	39–74
Mixed Bloods: No. Cases	49	56	58	47	51	54
Ave. Score	6.0	10.5	11.7	41.3	46.8	57.1
A.D.	2.4	3.4	1.3	8.8	10.0	6.3
P.E.	2.0	2.8	1.1	7.4	8.5	5.3
Median	5.4	9.8	11.2	40	47.3	59.8
Range	0–13	2–20	1–20	8–63	16–68	35–78
Per cent attaining median of Full Bloods	73	69	72	49	59	74

	Logical Memory Test			Rote Memory-Concrete Test		
Full Bloods: No. Cases	92	81	37	106	83	38
Ave. Score	14.7	25.6	28.2	37.5	39.9	43.1
A.D.	9.7	9.9	10.0	4.8	4.3	2.3
P.E.	8.2	8.3	8.5	3.9	3.6	1.9
Median	12.8	26.0	29.5	37.6	39.3	41.3
Range	0–36	4–46	8–51	22–58	26–54	32–56
Mixed Bloods: No. Cases	54	52	50	46	50	57
Ave. Score	22.5	29.7	35.3	37.7	41.3	45.4
A.D.	9.5	9.6	6.3	4.8	2.4	4.0
P.E.	8.1	8.1	5.4	3.9	2.0	3.4
Median	22.3	30.5	35.6	37.1	42.2	45.0
Range	0–50	9–55	13–50	20–51	26–54	33–59
Per cent attaining median of Full Bloods	79	67	86	50	66	82

TABLE II. — *Continued*

Sub-groups	Rote Memory-Abstract Test			Word-Building (eairlp) Test		
	I 4-5	II 6-7	III 8, 9-10	I 4-5	II 6-7	III 8, 9-10
Full Bloods: No. Cases	106	83	38	101	80	35
Ave. Score	27.1	32.2	38.8	10.1	12.9	15.9
A.D.	6.6	5.3	4.3	4.0	3.5	7.1
P.E.	5.6	4.4	3.5	3.3	2.9	6.1
Median	25.9	31.8	38.5	9.5	13.0	14.5
Range	6–51	17–47	24–51	0–21	0–22	0–22
Mixed Bloods: No. Cases	46	50	57	51	51	56
Ave. Score	28.9	35.2	41.9	10.4	13.3	16.5
A.D.	3.4	1.9	3.8	4.4	3.7	1.8
P.E.	2.8	1.5	3.1	3.7	3.2	1.4
Median	30.3	37.0	41.7	9.5	13.0	17.5
Range	19–48	22–49	27–54	0–20	1–21	0–22
Per cent attaining median of Full Bloods	80	82	75	57	51	71

Word-Building (aeobmt) Test.*

Sub-groups Grades	I 4–5	II 6–7	III 8, 9–10
Full Bloods: No. Cases	98	85	38
Ave. Score	11.1	13.2	15.2
A.D.	3.8	2.4	2.3
P.E.	3.1	1.9	1.1
Median	10.8	12.8	15.2
Range	0–20	2–23	5–23
Mixed Bloods: No. Cases	50	49	58
Ave. Score	10.0	13.2	15.8
A.D.	1.6	1.9	1.5
P.E.	1.3	1.5	1.3
Median	10.1	14.0	15.3
Range	1–17	2–21	1–22
Per cent attaining median of Full Bloods	52	67	58

blood groups as given in the Table II. In doing so let us have in mind several questions which we may ask:

1. Do they differ in their performance and how much if they do?

2. Do they profit by education and which group profits most? In this we take the sub-groups as representative.

3. Are any peculiar abilities possessed by either group?

4. In which test do they differ most?

5. Which test proved most difficult for a group?

6. Is there any evidence of superior mentality in either group as compared with the other?

In order to answer the first question as to differences in the performances in the several tests by the groups, we have to consider the central tendencies of the sub-group performances, and the overlapping of the two distributions of the measures in each sub-group.

We first take into consideration the average scores. For instance in the opposites in the fourth and fifth grade sub-group these are for mixed bloods 8.1 and for full bloods 6.7. The per cent of the former to the latter is 120. In the sixth and seventh grade sub-group this per cent of the average score is 123 and in the eighth, ninth and tenth grade sub-group it is 133. The average of these per cents for the three sub-groups is 125. In the genus species test these per cents. are 107, 115 and 109, and the average 109. Table III gives these per cents for the several tests. In only one test did the full bloods excel the mixed bloods in a single sub-group. (See the second word-building test, fourth and fifth grade sub-group.) The averages of the per cents for a single test, reading across, may be taken as comparative measures of the group performances. Since the average of these is 111 per cent, we may say that the mixed blood Indians excelled the full blood Indians by 11 per cent of their performance.

But we wish to know something of the overlapping in the several sub-groups for the several tests before putting our trust in this measure of a difference between mixed and full blood Indians on a basis of education. A table of the overlapping has been prepared. [See Table IV.] On the average the overlapping for the fourth and fifth grade subjects is 52.8 per cent, median 57 per cent; for the next sub-group or the sixth and seventh grade sub-group, it was 67 per cent, median 67 per cent; and for the last sub-group it was 73 per cent, medium 74 per cent. The trustworthiness of this measure of difference obtained above must be judged by these measures of overlapping, those for the last sub-group being more reliable than those of the first two and the first of all being the least reliable.

TABLE III

SHOWING PER CENT OBTAINED BY DIVIDING MIXED BLOOD AVERAGE SCORE BY FULL BLOOD AVERAGE SCORE

Test	Sub-Group 4–5	Sub-Group 6–7	Sub-Group 8, 9, 10	Total	Ave.	Rank
Opposites	120	123	133	376	125	8
Genus Species	107	115	109	326	109	6
Part-Whole	117	125	115	357	119	7
Free Ass'n	106	106	107	319	106	4
Logical Memory	150	116	126	392	131	9
Rote Mem. Con	100	103	104	307	102	2
Rote Mem. Abs.	106	108	108	322	107	5
Word Bld. 1	102	103	105	310	103	3
Word Bld. 2	90	100	103	293	98	1
Average Per Cent	111	111	112		111	

TABLE IV

SHOWING OVERLAPPING—PER CENT OF MIXED BLOODS ATTAINING AND EXCEEDING MEDIAN OF FULL BLOODS

Opposites	69	73	75
Genus Species	56	72	65
Part-Whole	73	69	72
Free Ass'n	49	59	74
Logical Mem.	79	67	86
Rote Mem. Con.	50	66	82
Rote Mem. Abs.	80	82	75
Word Bld. 1	57	51	71
Word Bld. 2	52	67	58
	—	—	—
Average	62.8	67	73
Median	57	67	74

TABLE V

	Mixed Bloods			Full Bloods		
	Sub-Group 4–5	Sub-Group 6–7	Sub-Group 8, 9, 10	Sub-Group 4–5	Sub-Group 6–7	Sub-Group 8, 9, 10
Opposites	100	149	222	100	145	201
Genus Species	"	143	191	"	134	186
Part-Whole	"	175	195	"	164	202
Free Ass'n	"	113	138	"	113	136
Log. Memory	"	132	157	"	174	192
Rote Mem. Con.	"	110	120	"	106	115
Rote Mem. Abs.	"	121	145	"	116	140
Word Bld. 1	"	128	158	"	127	157
Word Bld. 2	"	132	158	"	119	137
Average	100	134	143	100	108	126

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The next observation we make is that in both blood groups the average score increases as we proceed from the lower educational sub-groups to the higher. For, to illustrate with the average score of the full bloods in any test, as the Logical Memory test, if we take the average score of the fourth and fifth grade sub-group as a base, we have, taking the next two educational group averages in the test in relation to it, the following proportion: 100, 174, and 192. The second sub-group stands 74 per cent better and the third sub-group 92 per cent better than the first. A table (Table V) showing these proportions with the average score of the first sub-group as a base are given herewith. The averages of these numbers in all tests for each sub-group is also given. These read thus, 100, 134 and 143 for mixed bloods and 100, 122, and 141 for the full bloods. It can be seen here that though both blood groups show an increase of score the per cent of increase is more regular with the full bloods and that the second sub-group of mixed bloods shows a higher gain over the first than does that of the same full blood sub-group at this point, but in this particular instance only.

The third question, as to whether the full bloods possess any abilities in higher degree than the mixed bloods and vice versa, is worth considering and while endeavoring to answer this question we shall attempt to find answers for questions four and five along with it.

If we refer once more to Table III showing the per cent of mixed blood score on a basis of full blood, we will note again, but in this connection, that the fourth and fifth grade sub-group of the latter excelled the former in the second word-building test. This may have been due to practice for this test followed the first word-building test in its administration. But even so the full bloods profited more by the practice than the mixed bloods in this sub-group. Still observing the per cents in this sub-group we note that here the most difficult test for the full bloods must have been the logical memory test and that the easiest, barring the test where practice entered, was the concrete rote memory test. The first word-building test supposed to investigate ability in

invention was rather easy for them, but none of the numbers with the exception mentioned indicate any special abilities for this blood group as compared with the other one.

If we arrange the tests in ranking order according to these numbers just used we have this series, putting the easiest first:

For Full Bloods, Fourth and Fifth Grades.

Rank.	Test.
1.	Second Word-Building Test.
2.	Rote Memory-Concrete Test.
3.	First Word-Building Test.
4.5	Rote Memory-Abstract Test.
4.5	Free Association Test.
6.	Genus Species Test.
7.	Part-whole Test.
8.	Opposites Test.
9.	Logical Memory Test.

It would seem to be indicated here that tasks of an inventive sort are performed more easily than some others by full bloods along with those of retention of isolated symbols and that abilities of these sorts are closely followed by the free play of associations. However, there is little difference between the last named and the ability to control the association process in the part-whole test, though the other controlled association performances worked more slowly. Concrete rote memory, as may be expected, is easier for them than that of abstract rote memory. The logical memory test offers an interesting situation, however, being the most difficult one of the tests. Language difficulty was probably the handicap. All this has been said for the fourth and fifth grade sub-group. But how do the other sub-groups correspond to this ranking? We have measured this by obtaining correlations between this sub-group and the others and find a positive correlation of .88 for the sixth and seventh grade sub-group and likewise a positive correlation of .94 for the eight, ninth and tenth grade sub-group, the correspondence being slightly better for the last one.

When we come to a consideration of high scores as indicators of superior performers, we may say that in the lowest educational sub-groups a full blood Indian obtained the highest score in all the tests but the logical memory and part-whole tests. In the opposites test the honors were even. The situation was not quite so good for them, *i.e.*, the full bloods, in the results for the second sub-group, for here the highest score was obtained by some members of their groups in the two word-building tests only; however, members of their sub-group equalled the best performers of the mixed group in free association and concrete rote memory. The highest score for all performers in the third sub-groups and in what proved the most difficult test for the full bloods, the logical memory test, was obtained by a member of that blood group. In this sub-group the full bloods excelled the mixed bloods again in the part-whole and second word-building tests and equalled them in the first word-building test. In the other five tests they fell below the highest score of the mixed bloods. That is, the highest scores in all the nine tests in all the sub-groups making twenty-seven instances were obtained twelve times by mixed bloods and eleven times by full bloods. In the four remaining cases they tied.

On the other hand the lowest scores were obtained by full bloods twenty-five times out of twenty-seven.

While it was impossible to measure the influence of social status on the results given here because the subjects, or students participating in the experiment, are gathered from points at great distances from the school, some of them more than 1,000 miles away, we feel that this factor was probably very large for the reasons given above in connection with our discussion of the influence of environment. It is likely that a home in which there is one white parent will supply a pressure toward civilized beliefs and behavior not to be found in a pure blood Indian home. In many cases the 'old folks' deprecate the departure of the pure blood youth from his home to the government school. Naturally they cannot view the disintegration of their primitive culture in a disinterested manner. Those children who do not go to school when the

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opportunity is offered stay at home and perpetuate the traditions of their fathers, being unable to withstand that social pressure. The frequent return of school graduates 'to the blanket' on the reservations is another evidence of the negative effect of the primitive social pressure. Tenacity of these old traditions among Indians is great. For instance, in a certain pueblo there is an adobe mission of some proportions which was built three hundred years ago and where the inhabitants throng to attend the service, yet the 'keevah,' a secret place for native religious ceremonials, is also kept intact in the midst of that pueblo and the sound of the tom-tom is dear to their hearts. Even though we find in the homes the kitchen cabinet and cookstoves, nevertheless the Indian mother, to the delight of her children, still makes Indian bread and other delicacies.

In general we wish to say:

While the results show that the mixed blood group is 11 per cent better than the full blood group in performance in spite of the fact that the latter are older generally than the former, the difference may not be racial after all but only nurtural. The pure blood carries habits of mind running more or less at cross purposes with each other, *i.e.*, habits of civilized thinking and attitude and of Indian thinking and attitude. The mixed blood processes do not suffer as much from these interferences, we believe, but they run more smoothly. Placing blood beside blood we find a full-blood scoring as well as or better than the mixed blood in about half the instances in spite of this possible conflict between the two ways of thinking. And before consigning the full blood Plains and Forest Indian germplasm to the abyss of hopeless inferiority we must consider duly what it can do as here indicated by the performance of these trustees of that germplasm. When it, Indian germplasm, can run the gamut of these psychological scales under such adventitious circumstances, the question remains, what could it do under more favorable conditions of culture?

In conclusion, our results show that under these conditions of uncontrolled social status but controlled school training:

1. The mixed blood Indians excel the full bloods by about 11 per cent in performing tests of higher mental processes.

2. Increased education produces an increase in score in these tests for both blood groups.

3. The average score of the mixed bloods is always above that of the full bloods excepting in a single instance which may be attributed to practice showing up in the latter group.

4. The easiest tests for the full bloods in terms of mixed blood performance were tests of inventiveness and of immediate memory, the concrete variety being slightly easier than the abstract.

5. Probably on account of language difficulty the logical memory test proved the most difficult test for the full bloods.

6. The other tests found particularly difficult for them were those of controlled association.

7. The best scores were about equally divided between the mixed and full blood Indians.

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DISCUSSION

INDUCTION AND RADICAL "PSYCHOLOGY"

Several interesting and thought-provoking papers[1] have recently appeared in this REVIEW, in THE YALE REVIEW and elsewhere, in which the writers attempted to psychoanalyze radicalism, as it were, and determine the real motives, conscious and unconscious, of the active and avowed enemies of the existing order of things. Some of the points made by these analysts were undoubtedly well-taken and deserve the attention of earnest students of revolutionary or reform movements. But to the present writer it seems that too much of the current discussions of radicalism is of the *a priori* sort, and that induction does not play in the inquiries the rôle it ought to play.

In other words, while admitting that disappointed, thwarted ambition, arrested purpose, resentment, a sense of personal grievance against society are factors in the making and feeding of radicalism, the present writer, a life-long radical and associate of radicals, as well as a student of past radical literature, is convinced that 'induction,' or an actual, adequate study of the lives and circumstances of radicals, great and small, destructive, quasi-constructive and truly constructive, would disclose the presence of other important factors, factors society cannot afford to ignore or belittle.

Just as it is not true that all critics are persons who tried creative work and failed, so is it far from being true that all, or even the majority, of radicals are persons who regard themselves as victims of nature or society—men and women of too limited opportunities, of insufficient ability to cope with their fellow-men in a fair and free field, or victims of class and caste prejudice and unjust legal systems.

The scientific method would suggest the verification of working theories, or of seemingly trustworthy general impressions, by careful studies of at least several hundred radicals.

No such study has ever been made, but, drawing on his own

[1] See especially: Stuart Paton, The Psychology of the Radical, Yale Review, Oct., 1921. A. B. Wolfe, The Motivation of Radicalism, Psychol. Rev., **28**, 1921.

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experience and reading, the writer ventures to submit the following facts and considerations.

1. A good many college boys and girls are known to be radical. These read Wells, Shaw, Nietzsche, Ibsen, Freud, and the advanced periodicals which reflect radical teaching, and apply it to current and concrete problems. They declare themselves to be insurgents in politics, economics and morals. Now, youth is proverbially impressionable, impulsive, eager, ardent, impatient of restraints and conventions, determined to try conclusions for itself with nature and humanity. There can be no gainsaying the proposition that the children of the well-to-do and comfortable are as apt to embrace radical ideas as the children of the poor. We cannot expect youth to be critical, to suspend judgment on vital and absorbing questions until all evidence available has been considered and weighed. Balance, poise, sobriety in reaching and entertaining conclusions are the characteristics of maturity, products of ample vital experience. The young are naturally attracted by novelty and originality, and this propensity makes them radical in one or more directions—in art, or manners, if not in politics.

Take a concrete illustration. In Russia, for several decades, under czardom, most of the students in the universities, gymnasia, technical and other schools, were socialists and revolutionists. This temper and attitude meant constant peril, sacrifices, the giving up of careers, of wealth, of pleasure, of life even, in thousands of instances. Ambition, desire, instinct, prudent calculation pointed one way; a great cause, an ideal of justice, liberty and beauty, pointed the other way. Generosity triumphed over egoistic motives. Princes, the sons and daughters of noblemen, of wealthy merchants, of successful professional men, unhesitatingly renounced everything for the sake of a cherished ideal. What they rebelled against was tyranny and wrong which affected them but slightly, but which, they felt, inflicted cruel suffering on 'the masses,' the peasants and the workmen. 'Land and liberty' was their slogan, though they had land or wealth and plenty of liberty. They had luxuries and comforts, the delights of art and science. They could endure the follies and stupidities of the autocracy, or even profit by them. They would not accept privilege, however, because they had conceptions of national and international justice and progress, of the common good, of happiness for all instead of for a few. Of course, a certain amount of egoism is present in cases which appear to the superficial observer to embody the purest altruism. But this consideration does not

entitle us to claim that what common sense regards, and rightly, as self-denial, sacrifice, is due to thwarted ambition or resentment.

2. Clear-headed and stanch conservatives, or moderates, have recognized the fact that the existing social-economic system is still characterized, even after decades of considerable reforms, by many features that offend our sense of justice and right. There *is* undeserved poverty. There *is* chronic unemployment. There are slums and there is appalling ignorance. There is monopoly, unfair privilege, flagrant inequalities traceable to all manner of abuses under feudalism, or under autocracy, or under mixed institutions that, however unavoidable historically, are seen to be defective and largely anachronistic. Such evils as exist may not rationally justify destructive radicalism, since such radicalism instead of improving matters tends to render them worse. But they have led even such a man as the late Prof. Thomas H. Huxley to exclaim that, if he thought the present system could not be greatly improved, he should pray for a collision between the earth and some friendly planet, a collision that would end forever the career of our vale of sorrows and tears! We cannot wonder at the circumstance that conditions responsible for Huxley's cry of revolt, or call to constructive effort, should embitter others and lead them to declare war on the existing order.

The late Prof. William James classified minds, for philosophical purposes, into tough and tender species. We may classify minds, as well as temperaments and emotional natures, for purposes of sociological and ethical discussion, in the same way. Some are able to control their emotions, to discipline themselves, even while determining to fight injustice and wrong with all their might. Others, in the same situation, lose poise and balance and become "irreconcilables" of one type or another. The explanation of thwarted ambition or personal disappointment is totally gratuitous in many such cases. The law of parsimony should rule it out. Zeal, enthusiasm, lack of native sobriety, passionate hatred of evil and wrong sufficiently account for the radical extremists, at least as a class.

The revolutionary, destructive radical is intolerant. So is the bone-dry Prohibitionist. So is the tory or bourbon capitalist who can see no virtue in any labor organization and to whom the whole trade-union movement is simply a senseless protest against economic law, a protest instigated and incessantly stimulated by self-seeking labor demagogues, grafters and loafers! There are fanatics and

rabid extremists in every class, not excepting the self-styled ultra-conservatives, and there are persons of deliberate, careful, cautious habits in the most radical camps.

How are opinions formed, and how do they ripen into convictions? Psychology might well attack this two-fold problem in earnest and help solve it. As a matter of fact, the most judicious of men, and the most studious and methodical, often reach conclusions, and steadfastly adhere to them, not because they have duly considered all the pros and cons of the issue involved—say protection *vs.* free trade, socialism *vs.* individualism, abolition of capital punishment *vs.* retention of the extreme penalty—but because of some unconscious bias, some intellectual 'affinity,' some predisposition, or 'will to believe,' only partly reënforced by study and thought. What individualist can claim that he has analyzed and weighed *all* the arguments, to date, that have been advanced by the several schools of socialism? What protectionist can claim that he knows the whole case for free trade and has after searching consideration of it decided for protection? These illustrations might be multiplied indefinitely.

If, then, the process of opinion-formation is largely mysterious, how dangerous it is to indulge in sweeping generalizations regarding the 'motivation' of radicals! There are as many motives prompting radical beliefs and actions as there are prompting liberal, conservative and reactionary beliefs and behavior.

The safe course to pursue, to repeat, in dealing with the question is to study several hundred cases, past and present. Take such radicals as St. Simon, Fourier, Godwin, Proudhon, Blanc, Shelley, Ruskin, Morris, Reclus, Kropotkin, Bakounin, Marx, Lassalle, Thoreau, Hyndman, Shaw, Wells, Jaurès, Lenin, on the one hand, and then take typical representatives of the rank and file in the Socialist, Communist, Syndicalist, Anarchist, Guild-Socialist and Advanced-Single Tax groups. Study their conditions, lives, connections, temperaments.

In the light of sufficient and trustworthy data of the sort indicated it would be possible to discuss with some degree of assurance the springs and motives of radical behavior, radical warfare on modern civilization.

The writer is of the opinion that radicals have more than their fair share of generosity, of altruism, of good sense and of capacity for self-criticism. But he may be wrong—and is quite willing to abide by the results of scientific investigation.

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